ESSAYS ON THE ESTIMATION OF PRICES IN IMPLICIT MARKETS

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Chapter one examines workers' labor supply decisions to estimate both the equilibrium compensating differential for fatal risk and marginal willingness to accept fatal risk. Using new panel data from commercial fishing deckhands in the Alaskan Bering Sea between 2003-2009, I exploit variation within the worker-firm level in hourly earnings across fishing seasons and exogenous variation in weather-dependent seasonal fatality rates to identify the compensating differential for occupational fatality risk. This identification approach avoids many of the common sources of bias in the estimation of compensating differentials, including endogenous job switching and unobserved worker and firm heterogeneity. Additionally, changes in earnings caused by exogenous changes in risk within the worker-firm level identify information about individual preferences for risk that is not conveyed by the equilibrium wage-risk envelope. A structural labor supply model incorporates dynamic job-specific skill accumulation, which affects subsequent earnings. The compensating differential, normalized to a value of statistical life, is estimated to be $6.17 million using a reduced-form model and $6.75 million using a structural model. Estimates from both models suggest that wages increase as a concave function of the level of risk. The findings imply that aversion to marginal increases in fatal risk falls as the level of risk rises, and that the marginal VSL is a decreasing function of the level of risk.

The second chapter uses unique data from a survey of physicians in 5 states to provide the first comprehensive analysis of the effects of non-compete agreements on labor markets. We estimate factors associated with the use of non-competes, the compensating wage differential for accepting a non-compete clause, and discuss the effects that non-compete clauses have on mobility and the geographic distribution of primary care physicians. We find that non-competes are used frequently among primary care physicians, and that they impose binding constraints on labor market behavior and are associated with significant compensating wage premiums of about 11% of wages. The analysis suggests that state-level public policies play an important role, as we find that states with very strict enforceability of non-compete agreements have fewer physicians per capita and higher prices for physician services.

BIOGRAPHICAL SKETCH

Kurt Lavetti grew up near the Chesapeake Bay in Southern Maryland. Eager to escape, he moved north to attend Boston College in 2000. After studying environmental economics at the London School of Economics during the summer of 2002, he returned home and was inspired to switch his major to economics and write his senior thesis on the economics of climate change. He graduated in 2004, and took a job at a consulting company in Boston, spending most of his days working in antitrust economics and most of his evenings perfecting his palette for inexpensive wine, instigating impromptu dance parties, and the like.

While following the debate in Massachusetts on health insurance reform, he soon became interested in health economics and moved to Ithaca to study economics at Cornell. The moment of luck that changed his graduate career was the receipt of a grant to travel to Unalaska, Alaska, and collect data to turn an improbable idea into this dissertation. He completed his Ph.D. in the summer of 2011, and looks forward to spending two years at the University of California, Berkeley, as the Robert Wood Johnson Scholar of Health Policy.

to always going full speed,

ahead on occasion

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CHAPTER 1

ESTIMATING COMPENSATING DIFFERENTIALS AND PREFERENCES FOR OCCUPATIONAL FATALITY RISK

1.1 Introduction

A substantial body of theoretical and empirical literature suggests that labor markets reward workers for accepting fatal risk.[1] However, the collective empirical evidence on the size of equilibrium compensating differentials for fatal risk is extremely imprecise. This imprecision is largely a consequence of two types of endogeneity biases: biases caused by unobserved differences across workers and across firms that are correlated with observed characteristics in a hedonic wage model, and bias caused by the endogeneity of variation in wage-risk pairs across job-spells. The equilibrium wage-risk locus is primarily driven by assortative matching, in part on unobserved dimensions.[2] For example, workers whose unobserved characteristics allow them to earn higher wages are likely to choose jobs with more desirable non-wage characteristics, causing bias in a hedonic wage model. The use of panel data to overcome biases caused by the omission of worker characteristics, by identifying the compensating differential from job switches, has exacerbated another type of bias caused by the endogeneity of the decision by a worker to switch jobs.[3] Moreover, data limitations have prevented panel approaches from controlling for unobserved heterogeneity across firms, as this would require variation in measured fatality rates

[1] See Viscusi and Aldy (2003) for a macro review of this literature.

[2] See, for example, Krueger and Summers (1988), Murphy and Topel (1987, 1990), Gibbons and Katz (1991), Abowd, Kramarz, and Margolis (1999), and Kniesner, Viscusi, and Woock (2010)

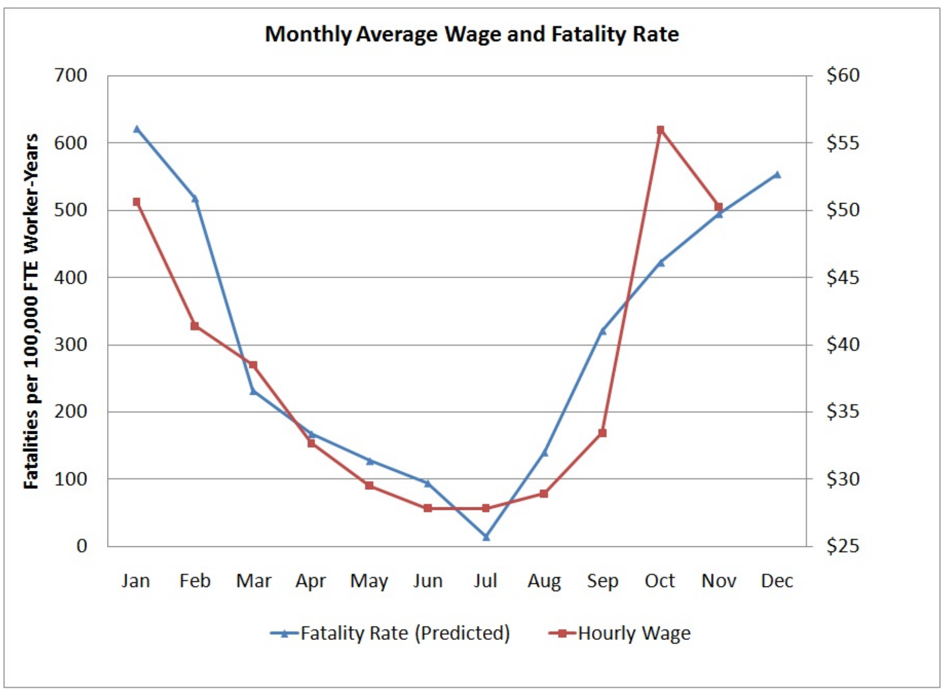
[3] See Gibbons and Katz (1991) for empirical evidence suggesting that endogenous job mobility biases estimates of compensating differentials.

within the firm level.

In this paper, I use a unique new panel data set to eliminate these two most significant econometric problems that have biased every empirical estimate of compensating differentials. The estimation approach controls for unobserved heterogeneity in worker characteristics, firm characteristics and worker-firm match effects. In doing so, it separately identifies the impact of assortative matching on the equilibrium wage-risk envelope and workers' marginal willingness to accept (MWA) occupational fatal risk. The MWA, unlike the equilibrium compensating differential, is a characteristic of workers' fundamental preferences for fatal risk. The application of preferences to the estimation of the value of statistical life can improve social welfare relative to the use of compensating differential estimates that are affected in particular by unobserved heterogeneity in firms' isoprofit functions in wage-risk space.

The data for this study come from a survey of commercial fishing deckhands who worked in the Alaskan Bering Sea and Aleutian Islands (BSAI) fisheries between 2003 and 2009. The labor market for commercial fishing deckhands has several unique characteristics that make it unusually well-suited for identifying the compensating differential for occupational fatality risk. Deckhands' earnings are determined by revenue-sharing contracts, so seasonal changes in output prices directly cause variation in hourly earnings. Also, seasonal changes in weather conditions cause exogenous and predictable changes in the fatality rate. Fishing vessels negotiate forward contracts with processors, which specify a quantity, unit price, and delivery date, before the start of a season. Since fatality rates are predictable ex ante, fishing vessel captains demand output prices that are sufficiently high to compensate their crew for the expected level of fatality risk. Consequently, fatality rates and hourly earnings vary substantially across fishing seasons, even for workers who remain employed at the same firm. The variation in monthly average hourly earnings and fatality rates is shown in Figure 1.1. I make use of this variation within the worker-firm level to remove bias from static unobserved characteristics of workers, firms, and match effects. The exogeneity of weather changes that cause this variation in wage-risk pairs avoids bias from endogenous job switches that was present in other panel studies.

Figure 1.1: Monthly Average Fatality Rate and Hourly Earnings



Sources: Survey Data, AOISS Fatality Data, NIOSH FTE Workers Data, NOAA Weather Data

The inveterate problems in the estimation of compensating differentials are largely due to data inadequacies. Nearly all of the US labor market studies of the compensating differential for fatal risk rely on cross-sectional data on workers and firms, and industry-level average fatality rate data. The large unexplained inter-industry and inter-firm wage differentials found in many studies, such as Krueger and Summers (1988), Groshen (1991), and Abowd, Kramarz, and Margolis (1999), suggest that unobserved firm and industry characteristics explain a substantial amount of the variation in wages.[4] Controlling for the unobserved differences across firms and industries in a cross-sectional hedonic wage model is precluded by the measurement of fatality rates at the industry level. The pursuit of more complete observable characterizations of workers and firms has proven to be insufficient to alleviate concerns about omitted variable bias.[5]

Two studies, Brown (1980) and Kniesner, Viscusi, and Woock (2010), use fixed effects models to reduce bias from static unobserved worker characteristics. Identification of the compensating differential comes from changes in wage-risk pairs for workers who switch jobs across industries. Since fatal risk is generally measured at the industry level, this identification approach introduces bias by restricting the sample to those workers who made the potentially endogenous choice to switch jobs and switch industries. In addition, bias from the omission of unobserved firm effects and worker-firm match effects remains.

Removing bias from the estimation of the compensating differential for fatal risk is particularly important because these estimates have been used to infer estimates of the value of statistical life (VSL). The VSL is simply a normalization of the compensating differential for fatal risk to a level of risk equal to one expected fatality. The White House Office of Management and Budget requires federal agencies to

[4]Krueger and Summers estimate the standard deviation of industry wage differentials to be greater than 10% after controlling for occupation, human capital, and demographic factors. Groshen (1991) estimates the standard deviation of establishment wage differentials to be about 14%.

[5]See Viscusi and Aldy (2003), Purse (2004), and Bonhomme and Jolivet (2009).

explicitly use the VSL in the benefit side of cost-benefit analyses for safety policies. Therefore, even small biases in estimates of the compensating differential can have large effects on public resource allocation. There are other market-based approaches to estimating the VSL, which have very different estimation concerns, but this paper focuses on improving labor market estimation of compensating differentials. Ashenfelter (2006) describes the inability to isolate an exogenous event that affects both wealth and fatality risk as the most important issue concerning the estimation of the value of statistical life (VSL).

In this study, I use fatality data collected by the Alaska Occupational Injury Surveillance System (AOISS), which include individual fatal events, rather than industry aggregates. The disaggregated nature of the data reduces measurement error, and permits the estimation of seasonal changes in fatality rates, and the matching of contemporaneous weather conditions to the date and location of fatal accidents.

Data inadequacies have not only prevented unbiased estimation of compensating differentials, they have also curtailed the scope of questions that empiricists have been able to ask. Labor-market estimates of the VSL that are used to form public policy should reflect the marginal willingness of workers to accept fatal risk, which is a fundamental preference. Researchers have instead estimated the market equilibrium relationship between wages and fatal risk, which contains almost no information about the underlying preferences of workers. This fact was articulated by Rosen (1974) in his seminal paper on the theory of hedonic prices and implicit markets. The differences between equilibrium wage-risk pairs and workers' preferences for risk are caused by heterogeneity among firms in the cost of providing safety, and the consequent assortative matching of workers and firms. Hwang, Mortensen, and

Reed (1998) demonstrate using a model of labor market search that hedonic wage models produce biased estimates of workers' marginal willingness to trade wages and non-wage amenities when firms are heterogeneous. Bonhomme and Jolivet (2009) provide empirical evidence that supports this theory in the context of subjective job characteristics, not including fatality risk.

The difference between labor-market estimates of the preference-based VSL and the equilibrium-based VSL is caused in part by heterogeneity in firms' cost functions. In the evaluation of the benefits of an optimal public policy, the cost functions of firms should have no impact. Consequently, basing public policies on VSLs derived from equilibrium compensating differentials is suboptimal.

A standard assumption in the majority of the empirical literature is that compensating differentials for fatal risk increase linearly with the level of risk, which implies that the VSL is constant, independent of the level of risk. This strong assumption about the equilibrium wage-risk functions was described by Ekeland, Heckman, and Nesheim (2004) as 'arbitrary and misleading.' I relax the linearity assumption to estimate the VSL as a nonlinear function of the level of risk, and find the VSL to be a decreasing nonlinear function of the level of risk. That is, the results suggest that aversion to marginal increases in fatal risk falls as the level of risk rises. By controlling for the effects of firm heterogeneity and assortative matching on the equilibrium function, this result is an estimate of the marginal willingness of workers in the sample to accept fatal risk. Specifically, I find that the VSL decreases from $6.17 to $2.43 million when the fatality rate increases from 70 to 500 fatalities per 100,000 full-time equivalent worker-years.

Policymakers depend on VSL estimates from labor markets to assess the benefits of safety policies. Since policy options often have very different levels of risk than the labor markets in which compensating differentials are estimated, the use of labor market estimates requires the strong assumption that the VSL is independent of the level of risk. Understanding how the marginal willingness to accept risk depends on the total level of risk accepted will allow policymakers to more accurately tailor public policies to reflect the preferences of the affected population. Adjusting the VSL according to changes in marginal preferences is especially important for policies that have high levels of risk relative to average occupational risks, and for policies that affect populations uniformly, since people cannot then sort themselves according to their preferences for risk, as they can in labor markets.

Another advantage to estimating the VSL in this empirical setting is that the support on which fatality rates vary is extremely large, extending to over 600 fatalities per 100,000 full-time equivalent (FTE) worker-years, which is more than 150 times larger than the average US civilian workplace fatality rate. This unusually large amount of variation in risk facilitates the nonlinear estimation of the functional relationship between the VSL and the level of fatal risk. Although the level of risk is unusually high relative to other labor markets, there are many examples of policies that govern the acceptance of similar levels of risk. For example, the mortality rate attributable to smoking among men in the US is 383.9 per 100,000 person-years, which is higher than the average level of risk in BSAI fisheries.[6]

To summarize the advantages of the empirical approach taken in this paper, relative to the approaches taken in literature: (1) I collect a new and unique set of labor-market data, using a survey that was designed for estimating risk-wage tradeoffs in

[6]Source: CDC. State-Specific Smoking-Attributable Mortality and Years of Potential Life Lost — United States, 2000–2004. MMWR 2009;58:29-33.

a panel setting; (2) the use of panel data on changes in wages and fatality risk within the worker-firm level mitigates omitted variable bias by controlling for unobserved individual characteristics, unobserved firm characteristics, and unobserved match effects; (3) exogenous variation in wage-risk pairs within worker-firm level avoids bias caused by the potentially endogenous choice to switch jobs; (4) I identify the marginal willingness of workers to accept fatal risk by controlling for heterogeneity in firm productivity and unobserved worker-firm match effects; (5) the support over which fatality risk is observed extends to more that 150 times the average US manufacturing fatality risk, providing an unusually large amount of variation in risk, and facilitating the estimation of nonlinear affects of fatality risk on wages; (6) fatality rate data come from a new source, the Alaska Occupational Injury Surveillance System, which very thoroughly reports the details and circumstances of each fatal event in the Alaskan commercial fishing industry; and (7) using data on individual fatalities, rather than industry aggregates, reduces concern about measurement error, allows for all of the variation in fatality rates to be used for estimation, and allows one to estimate exogenous seasonal changes in fatality rates. These improvements address the majority of the remaining concerns in the literature regarding the econometrics of estimating compensating differentials, and provide the most complete labor-market estimates yet of preferences for fatal risk.

The paper proceeds as follows: Section 2 describes the current state of knowledge about the compensating differential for occupational fatality risk. Section 3 provides an overview of the industry and institutional labor market details that are relevant to the model and estimation. The survey and other data are discussed in Section 4. A structural model of dynamic labor supply with job-specific skill accumulation is presented in Section 5. Structural and reduced-form results are presented in Section 6. Section 7 discusses the implications of nonlinear values of statistical life and preference-based approaches for the evaluation of public policies. Section 8 concludes.

1.2 Existing Evidence on the Compensating Differential for Fatal Risk

Viscusi and Aldy (2003) review the literature on the estimation of compensating differentials for fatal risk in US labor markets. They describe 32 studies and summarize estimates, normalized to VSL measures, as typically falling within the range of $3.8 to $9.0 million, although the majority of the studies they review find estimates outside of this range. More recently, Kniesner, Viscusi, and Woock (2010) describe the state of knowledge about US labor market estimates of the VSL as ranging "from about $0 to $30 million," attributing the wide range of estimates to econometric issues.

Of the 32 studies in the Viscusi and Aldy review, only one uses panel data to control for unobserved individual heterogeneity. The primary reason for the nearly exclusive use of cross-sectional data is the scarcity of available data with sufficiently long panels to observe job switches. It is also unclear whether there is any benefit to using panel data to attenuate omitted variable bias when doing so exacerbates bias caused by endogenous job switches. Brown (1980), estimates the compensating wage differential for fatality risk among young men in the NLSY who switch jobs across industries. He focuses on young men in part because they are more likely to switch jobs, which is necessary to identify unobserved worker effects. Brown finds inconsistent evidence of a significant compensating differential for fatality risk, and attributes this, in part, to the inability to observe enough information about jobs. In addition to the inadequacies of labor market data, the available data on fatality risk have also been problematic for the estimation of compensating differentials.[7]

The only other longitudinal study, and the only panel study to find a robust statistically significant compensating differential for fatality risk using US labor market data, is Kniesner, Viscusi, and Woock (2010). Their estimates, which use the longitudinal structure of PSID data to identify the compensating differential from workers who switch industries, range from about $7 to $12 million. Following the identification approach used by Brown (1980), the authors use panel data to control for latent static heterogeneity across workers. They describe bias from unobserved heterogeneity as the most important econometric issue in the estimation of compensating differentials.

[7]Measures of fatality risk have come from either objective fatalities data or subjective self-reported risk from worker surveys. Of the US labor market studies of the risk-wage tradeoff, all but three have relied exclusively on some combination of data from the Bureau of Labor Statistics (BLS), the National Traumatic Occupational Fatalities (NTOF) surveillance system, collected by the National Institute of Occupational Safety and Health (NIOSH), or the American Society of Actuaries (ASA) to construct an objective measure of fatality risk. The vast majority of these studies used BLS data, which are industry-specific risk measures available since the late 1960s. Leigh (1989) argues that the publicly available BLS data are biased because they only record data on firms with more than 10 workers, and other estimates suggest that about two-thirds of all workplace fatalities occur at small firms with fewer than 10 workers. Moreover, prior to the early 1990s BLS data were only available at aggregated 2 or 3-digit SIC codes, raising significant concern about measurement error for this key variable. Since 1992 the BLS has collected occupational injury data through the Census of Fatal Occupational Injuries (CFOI), which aggregates fatality rates to the 4-digit SIC level. NTOF data have been available since 1980, and rely upon reported cause of death on death certificates. NTOF data suggest fatality risks were about 50% higher than BLS data during the 1980s. Still, comparisons between BLS data and NIOSH data reveal substantial differences in risk.[8] ASA data are available for a very limited number of occupations, and the risk measures differ from measures in the BLS and NIOSH data by almost an order of magnitude for some occupations. The difficulty in accurately estimating the level of fatal risk that workers face has contributed to the uncertainty about the VSL.

Linearity of the wage-risk locus has been assumed by all except a few studies in the literature,[9] which have included quadratic functions of fatality risk in wage models. These studies have all found the squared term to be negative and significant, suggesting that the equilibrium wage-risk locus is a concave function. However, in the cross-sectional studies that found this result, it is impossible to separately identify the extent to which the estimated concavity is due to assortative matching caused by unobserved worker and firm heterogeneity, as opposed to individual preferences for risk. Theory suggests that workers with lower aversion to fatal risk should sort into riskier jobs, which is sufficient to produce a concave equilibrium function.[10]

One paper specifically discusses the shape of preferences for fatal risk, but provides very limited empirical evidence on the subject. Viscusi and Hersch (2001) estimate the compensating differential for non-fatal workplace injuries for smokers and non-smokers in an attempt to understand heterogeneity in preferences for fatal risk. They find that smokers work in riskier jobs but receive lower wage compensation for the risk they accept than do non-smokers, and they discuss the implications of this finding on the shape of the wage-risk offer curves faced by smokers and non-smokers. The authors reach the conclusion that smokers must face different market opportunities than non-smokers, as the difference in equilibrium outcomes cannot be explained by differences in preferences alone. The simultaneity of differences in market opportunities and potential changes in preferences that are correlated with smoking leaves insufficient variation to identify preferences for risk.

Another study of relevance to this paper is Schneier, Horrace, and Felthoven

[9] Viscusi (1981), Leigh and Folsum (1984), Olson (1981), and Dorsey and Walzer (1983)

[10] Viscusi (1992)

(2010). This paper does not estimate the compensating differential for fatality risk, but it does estimate the VSL using a non-labor-market approach based on fishing vessel captains' decisions, as agents of their firms, to risk the lives of their crew by fishing on a given day conditional on observed short-term weather conditions. Focusing on the Red King and Snow Crab fisheries, which are represented in the data that I use, they estimate the VSL to be about $4.0 to $4.8 million.

1.3 Industry Background

Fisheries and related seafood processing industries are a vital part of the Alaskan economy, especially in southwest Alaska, where seafood harvesting (20.9%) and seafood processing (31%) jobs account for more than half of all private-sector jobs.[11] The economic dependence upon seafood is even stronger in certain processing ports. Dutch Harbor, a port in the Aleutian Islands, is not only the largest seafood processing port in Alaska, but has also been the largest seafood port by volume in the United States for more than twenty years.

The captains of fishing vessels in the BSAI fisheries hire deckhands and compensate them with revenue-sharing agreements. Revenue-sharing levels increase sharply with initial experience, after which they increase more slowly. The effects of commercial fishing experience on revenue-sharing levels for various species are presented in Table 1.1. Revenue-sharing levels vary according to the species fished and the number of deckhands on a vessel. There are also idiosyncratic differences in average revenue-sharing levels across vessels. These differences could be due

[11] Source: Robinson, Dan and Neal Gilbertson, "Fish Harvesting Employment," Alaska Economic Trends, December 2006.

to unobserved heterogeneity in firm productivity, unobserved worker-firm match productivity, or differences in the types of workers hired by different firms.

The population of commercial fishermen discussed in this paper includes men between the ages of 22 and 55 who either lived in, or worked on fishing vessels that conducted business in, Dutch Harbor between 2003 and 2009. Workers in the population are very geographically diverse, and they travel to Dutch Harbor for fishing job spells. About 20.7% of the population of workers were estimated to live in the state of Alaska, 64.5% lived in either California, Washington, or Oregon, and the remainder were almost entirely from other US states.[12] Most commercial fishing deckhands held another job in addition to fishing. Typically, non-fishing jobs are seasonal and of short-duration, although the season spent away from fishing varies by individual.

The analysis in this paper focuses exclusively on deckhands, as opposed to other vessel crewmembers, such as captains. The primary reason for excluding captains is because they have the ability to make and revise expectations about earnings and fatal risk continuously during a fishing trip, and they have the authority to change decisions based on changes in their expectations. For example, a captain can steer the boat to fish in safer conditions at the expense of expected revenue. Deckhands, however, commit to working on the vessel for the duration of a fishing season, and have less ability to change their expected earnings, expected fatality risk, or labor supply decision once the vessel is at sea. To the extent that captains have the ability to control both expected earnings and expected fatality risk continuously, and because the marginal tradeoffs they make are not observed by researchers, an estimate

[12]Author's calculation based on longitudinally linked sales data of commercial fishing licenses provided by the Alaska Department of Fish and Game.

Table 1.1: Revenue-Sharing Contracts and Fishing Experience

Dependent Variable: Percentage of Vessel Net Revenue Earned by Worker				
	Shellfish	Cod & Pollock	Other Groundfish	Salmon
Years Fishing Experience	0.231 ***	0.184	1.150 ***	0.642 ***
	[0.061]	[0.191]	[0.288]	[0.232]
Years Fishing Experience Sq.	-0.009 *	0.013	-0.081 ***	-0.030 **
	[0.005]	[0.017]	[0.018]	[0.015]
Years Fishing Experience Cu.	0.000	-0.001 *	0.001 ***	0.001 **
	[0.000]	[0.000]	[0.000]	[0.000]
Constant	3.882 ***	3.779 ***	4.487 ***	8.011 ***
N Obs.	716	244	201	120
R^2	0.106	0.157	0.132	0.376

* Significant at the .10 level, ** significant at the .05 level, *** significant at the .01 level

of the price of fatal risk based on analysis of ex post realizations of earnings and ex post fatality rates that includes captains may be biased.

Fishing and crabbing boats operate intermittently throughout the year during various regulated seasons. The reason for the intermittency is that the equipment used to catch different species is somewhat specific, so vessels are not completely fungible. Vessels establish forward contracts with processors before the start of a season to deliver a specified amount of catch for a fixed price on a predetermined date. The contracts often specify a schedule of substantial price reductions if vessels are late for delivery. This incentive, in addition to seasonal regulations and the cost-structure of operating a fishing vessel, cause deckhands to work very long hours, typically 18-20 hours per day. Most vessels have six or fewer deckhands.

Prior to 2005 the fisheries had either entry limitations or aggregate catch-limits, and vessels raced against each other to catch as much of the aggregate quota as possible. Beginning in 2005, transferable vessel quotas were introduced into the BSAI Crab fisheries, restricting the catch for each vessel. Many other BSAI fisheries remain without vessel quotas. This led to consolidation of vessel capital, and the exit of many vessels from the crab fisheries. Nonetheless, about 95 vessels remain in operation in the crab fisheries, which have the fewest vessels. The ownership of vessels is highly disperse, with most owners controlling a single vessel.

The turnover rate for crew members is very high, and generally decreases with experience. Most fishermen work fairly short careers to earn high short-term rewards. One reason workers tend to exit the industry so quickly is that seafood harvesting the southwest Alaska is among the most dangerous jobs the country. In 2006 the average annual rate of fatal work-related injuries in the US was 3.9 per 100,000 workers. Commercial fishermen, by comparison, had an average US fatality rate of 141.7 per 100,000 full-time equivalent workers in 2006, the highest of any industry in the country. Seafood harvesting in Alaska's BSAI fisheries in winter months typically has average fatality rates of over 500 deaths per 100,000 full-time equivalent workers.

More than 80% of fatalities in the BSAI fisheries occur due to hypothermia and/or drowning. Water temperature and wave height have strong causal impacts on fatality risk. Specifically, 74% of seasonal variation in long-run fatality rates in the data is explained by variation in long-run monthly average water temperature and average significant wave height. Figures 1.3 and 1.4 show monthly average weather conditions and fatality rates. The high level of risk in the winter months is caused by a combination of larger waves and lower water temperatures. One advantage to using this industry for estimating compensating differentials is that the risk varies substantially across seasons. Workers are reminded of the risk of death due to the frequency of accidents and publicity about risk levels, most notably from the television show *The Deadliest Catch*. The information necessary to forecast risk is highly visible since it is so strongly driven by seasonal weather conditions.

1.4 Data

1.4.1 BSAI Commercial Deckhand Survey

The labor market data used for this study come from a survey of men aged 22-55 who worked as commercial fishing deckhands in a BSAI fishery at any point between 2003 and 2009. The population of potential respondents was limited to those deckhands who worked on vessels doing business in Dutch Harbor, or deckhands who purchased a commercial fishing license from a vendor in Dutch Harbor. The survey was conducted in several rounds, via mail and direct surveys conducted in Dutch Harbor.

The survey includes questions on demographics, income, wealth, subjective risk perceptions, and recall-based panel questions dating back to 2003 on employment histories for all jobs, fishing and other. Regarding fishing-related employment, detailed questions were asked about each trip taken, including the month(s) during which the trip occurred, the species fished, the number of days spent fishing, the vessel name, all aspects of the labor contract, the average number of hours worked per day, and total earnings. Although concerns about the quality of recall data are warranted, there is some unique reason to be optimistic about the quality of recall data from this survey. Deckhands have been highly involved in the design, implementation, and review of the policy change through which the fisheries were transitioned from open-access to rights-based. In 2010 the policy was under review to determine whether it has had the intended effects. Workers were very aware of the changes that the policy had in the length of fishing seasons and earnings, and were easily able to make comparisons between the years before the policy change, 2003-2004, and those immediately after. For this reason, recall of 2003-2006 earnings did not seem to be problematic for respondents, and more recent data was timely enough to also recall.

Although the data collection is still ongoing, the data used in this draft of the paper include 133 respondents who worked a total of N=1351 fishing job spells. Of these respondents, 80 were in the direct survey group completed in October 2009, at the beginning of the Red King Crab season, and the response rate for this survey was 62.2%. The total population of deckhands in the BSAI Red King Crab fishery working on vessels departing from Dutch Harbor is estimated to be less 250, and the data contain more than 30% of the relevant population of deckhands who worked in this fishery in 2009. The remaining 53 respondents were from two separate rounds of mailing interviews, which had average response rates of 4.2% and 16%. The first mailing survey frame was derived from an Alaska Department of Fish and Game (ADFG) database of commercial fishing licenses sold, which provided the name, mailing address, and location of license purchase for every commercial fishermen in the state since 1988. The second mailing survey frame came from a database of vessels registered to participate in the 2010 Opilio Crab fishing season in January 2010. The surveys were mailed directly to vessels at the beginning of the season.

Table 1.2: Summary Statistics: Survey Demographics

Age	37.86
	[9.32]
Race	
White	81.11%
Black	1.57%
Non-White Hispanic	5.62%
Asian	5.32%
Other	6.37%
Education	
Less than High School Diploma	5.92%
High School Diploma/Equivalent	44.98%
Some College	38.08%
College Degree or More	11.02%
Ever Married	52.92%
At Least One Child	52.55%
Has Health Insurance	46.38%
Has Life Insurance	34.06%
State of Residency	
WA	49.85%
AK	21.35%
OR	6.47%
CA	4.14%
Other US	18.19%
N Respondents	133

The mean age in the sample is 37 years, with an interquartile range from 29 to 44 years old. The sample is 81% Caucasian, and about 83% of the sample has either a high school diploma or equivalent, or some college. Slightly more than half of workers are married, and about the same percentage have children. Workers are of diverse geographic origin, with only 21% coming from the state of Alaska, and about 60% from the Pacific Northwest states of California, Washington, and Oregon. The remainder of the sample was from other US states. Only 46% of survey respondents indicated that they have health insurance.[13] Of those that do have health insurance,

[13]This rate of insurance coverage is comparable to the average coverage rate in the Medical Expenditure Panel Survey for individuals who are not offered insurance by their employer and are not eligible for public insurance.

the most common source of the insurance is a spouse's employer.

Figure 1.2: Summary Statistics: Work Experience

		S.E.
Years F.T. Work Experience		
Mean	20.50	[9.96]
10th Percentile	7	
50th Percentile	20	
90th Percentile	35	
Years Fishing Experience		
Mean	16.12	[8.85]
10th Percentile	5	
50th Percentile	15	
90th Percentile	27	

On average, each respondent worked in 10.1 different fishing seasons between 2003 and 2009. About 70% of workers in the sample had a job other than commercial fishing during the panel. The mean hourly wage from outside jobs was $13.34, which was substantially lower than the mean fishing hourly wage of $33.28.[14] The mean annual earned income from all sources in the sample was $91,285. Additional summary statistics from the survey data on demographics, work experience, and earnings are reported in Tables 1.2, 1.2, and 1.3.

Table 1.3: Summary Statistics: Earnings

	Mean	S.E.
Percent Respondents with Nonfishing Job During Panel	70.24%	
Nonfishing Hourly Wage	$13.34	[$8.01]
Number of Fishing Seasons per Respondent During Panel	10.16	
Fishing Hourly Wage, All Years	$33.28	[$28.62]
Income per Fishing Trip	$34,980	[$28,627]
Hours Worked per Fishing Trip	1,051.16	[829.13]
Annual Income, All Sources	$91,285	[$58,731]

[14]The mean fishing wage weights job spells by their duration, measured in days.

To test the representativeness of the sample, I compare the sample means to the limited available data on the population, which come from the database of longitudinally-linked commercial fishing license sales. These population data include state of residency and the number of years in which each person purchased a commercial fishing license in the state of Alaska. A chi-square test of the geographic distributions of residents in Alaska, the Pacific Northwest, and other US states fails to reject that the sample is of different geographic origin than the population, with a p-value of 0.530. A chi-square test of the distribution of experience also fails to reject that the sample is significantly different from the population, with a p-value of 0.242.[15] The sample overweights fishermen who worked in crab fisheries, because the field survey in Dutch Harbor was conducted around the beginning of the Red King Crab season in 2009.

1.4.2 Fatality Data

Fatality risk data come from the Alaska Occupational Injury Surveillance System (AOISS), which is a database of every work-related traumatic injury and fatality in the state of Alaska. The AOISS is maintained by the National Institute for Occupational Safety and Health, and collects information on each injury and fatality from US Coast Guard reports, Alaska State Trooper reports, medical examiner documents, and death certificates. The NIOSH provided a subset of this database containing information on every commercial fishing fatality from 1990-2007. The data include the longitude and latitude of each accident, the circumstances of the

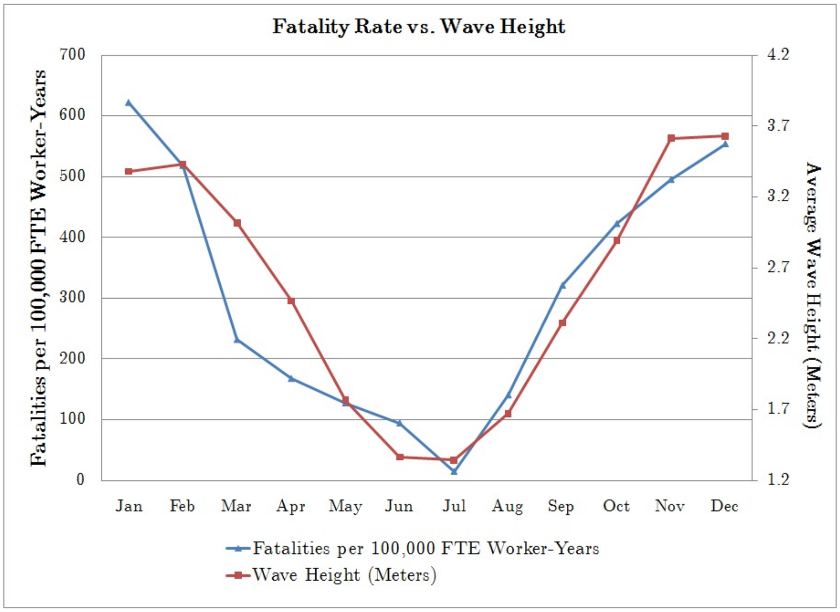
[15]The estimated population distribution of experience is truncated at 19 years, due to limitations in the commercial fishing license database. The comparison is made between the truncated sample and truncated population distribution of experience for values between 1 year and 19 years of experience.

accident, the date of the accident, vessel characteristics, the type of fishery, and the number of fatalities. Monthly fatality rates are calculated by combining AOISS fatality data with separate data from the NIOSH estimating the number of full-time equivalent workers in each Alaskan fishery in each year, and data from the Alaska State Department of Labor on the number of deckhands working in each fishery in each month of the year. The NIOSH data are used to adjust the monthly Department of Labor counts of workers to estimate the number of full-time equivalent workers.

1.4.3 Weather Data

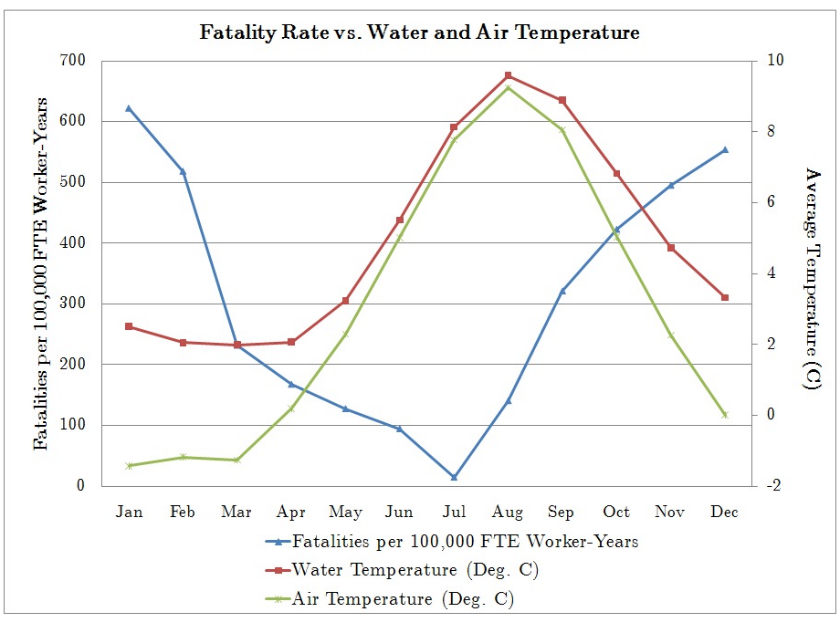
Historical weather data come from NOAA weather buoy station #46035, located in the Bering Sea. Hourly weather data are available dating back to 1985. The data used for this research include significant wave height and sea surface temperature. Figures 1.3 and 1.4 show monthly average weather conditions and fatality rates.

Figure 1.3: Fatality Rate vs. Wave Height



Sources: AOISS Fatality Data, NIOSH FTE Workers Data, NOAA Weather Data

Figure 1.4: Fatality Rate vs. Water and Air Temperature



Sources: AOISS Fatality Data, NIOSH FTE Workers Data, NOAA Weather Data

Seasonal weather patterns provide easily observable ex ante signals to workers about the fatality risk they face. Since more than 80% of fatalities are caused by hypothermia and drowning, the weather conditions that are most likely to cause fatal risk are waves, which cause deckhands to fall overboard or vessels to capsize, and water temperature, which affects the amount of time available for a successful rescue. Although the rate of fatality is extremely high, the Bering Sea fisheries are sufficiently small that ex post monthly fatality rates are very noisy. Since weather conditions are less noisy causal determinants of fatality rates, they are used to predict fatality rates.

Using the predicted fatality rate based on weather conditions may also be advantageous if fatality risk is endogenous in the wage equation. This could occur, for example, if a positive shock to the spot price for seafood induces deckhands to work longer hours or otherwise take more risks in an attempt to increase harvests at the higher price. The ideal IV to eliminate bias from the this type of endogeneity would be correlated with fatality risk, but uncorrelated with variables that covary with wage shocks. Long-run monthly weather patterns in the BSAI fisheries are useful IVs since they are strongly correlated with fatality risk but are likely to be uncorrelated with transitory shocks to the national or global markets for seafood.

The weather variables that are included in a model of fatality rates are average significant wave height[16], average water temperature, and the variance of wave heights in the month. Other weather variables are excluded because they are strongly correlated with the included variables, and have negligible additional explanatory power. The mean monthly weather conditions capture the central ten-

[16] Average significant wave height is defined as the average wave height, from trough to crest, of the one-third largest waves.

dency of each variable, while the variance term is included to explain the frequency of extreme weather events within each month, during which fatal accidents may be more likely.

Table 1.4: Negative Binomial Model

Negative Binomial Model, Zero-Adjusted	
Dependent Variable: Fatalities per 100,000 FTE Worker-Years	
Mean Average Significant Wave Height (M)	0.4795 ***
	[0.1598]
Variance Significant Wave Height	0.4310 ***
	[0.1470]
Water Surface Temperature (C)	-0.0550
	[0.0359]
N Obs.	180
R^2	0.484

Table 1.4 presents the results of the first-stage regression of fatality risk on the set of weather IVs. A negative binomial model is estimated, with adjustments for the frequent observation of zeros. The mean and variance of the distribution of wave heights are highly significant and explain a large proportion of the variation in fatality rates. Condition on the included wave height variables, water temperature has relatively little explanatory power.

1.5 Reduced-Form Estimates

Reduced-form estimation of the compensating differential for fatality risk typically involves using cross sectional data and estimating a model such as:

$$w_i = \alpha + \beta_1 H_i + \beta_2 X_i + \beta_3 p_i + u_i$$

where H_i includes individual characteristics, X_i includes job characteristics for worker i, and p_i is the industry average fatality rate. The analogous panel data approach is to estimate a fixed-effects model such as:

$$w_{it} = \alpha_{ij} + \gamma_t + \beta_4 p_{it} + u_{ijt}$$

In this model α_{ij} absorbs the effects of unobserved worker, firm, and match effects, allowing for unbiased estimation of β_4 in the presence of unobservable characteristics. γ_t controls for changes that effect wages uniformly over time.

Table 1.5: Linear Fixed Effects Models

	Dependent Variable			
	(1) *Wage*	(2) ln(*Wage*)	(3) *Wage*	(4) ln(*Wage*)
Fatality Risk	2.272 ***	0.066 ***	0.893 *	0.025 ***
	[0.376]	[0.009]	[0.433]	[0.009]
Worker Effects	Y	Y	Y	Y
Worker-Firm Effects	N	N	Y	Y
Constant	28.90 ***	3.148 ***	35.204 ***	3.331
N Obs.	1351	1349	1188	1186
N Clusters	133	133	184	184
R^2	0.446	0.613	0.516	0.762

Note:* Significant at the .10 level, ** significant at the .05 level, *** significant at the .01 level. † All models are weighted by the length of the job spell, measured in days. All models include year effects. Standard errors are clustered at the worker level in Models 1 and 2 and at the worker-firm level in Models 3 and 4.

Table 1.5 shows reduced-form estimates of models that are linear in the fatality rate. The fatality rate included in the model is measured as the number of fatalities per 1,000 full-time equivalent workers. Standard errors in both models are clustered at the individual level. The model is weighted by the number of days in each job spell, so that the longer job spells count more heavily than shorter spells. Model 1 is linear in wages and fatality rate, and suggest that for every increase of one fatality per 1,000 full-time equivalent workers the hourly wage increases by \$2.272. As a frame of reference, the average US manufacturing fatality rate is 0.039 fatalities per 1,000 FTE workers. The implied VSL is calculated by multiplying the measure of risk by the number of hours per full-time equivalent worker year, which is 2,000 hours. The VSL implied by Model 1 is therefore \$2.272 $\times$ $1,000 \times 2,000$, or \$4.544 million. If, instead, a log linear model is considered as in Model 2, the corresponding VSL estimate depends on the level of risk. When the risk level is 4 fatalities per 1,000 FTE workers, which is approximately the weighted sample average risk, the VSL implied by Model 2 is $4.003 million. Models 3 and 4 add worker-firm effects, rather than just worker effects, to control differences in firm characteristics. The VSL estimates are reduced to $1.786 million and $1.517 million, respectively.

1.5.1 Nonlinear Prices of Fatality Risk

In Models 1-4, as with most estimates in the literature, the possibility that equilibrium compensating differentials for fatal risk are highly nonlinear functions of risk levels is ignored. Table 1.6 shows the results of partitioned regressions similar to Models 1-4, which allow the compensating differentials to vary across quartiles of the distribution of fatality risks. In all four model specifications, the coefficients on fatality rate in the lowest quartile are higher than the coefficients in the middle quartiles, which are all higher than the coefficients on the highest quartile of risk. The hypothesis that the coefficients are all equal is strongly rejected in Models 5 and 6, but the standard errors of the coefficient estimates are quite large as a result of partitioning, and the null hypothesis cannot be rejected in Models 7 and 8, which include within worker-firm effects. Still, there is convincing evidence from the partitioned regression that it is worth investigating the possibility that the compensating differential for fatal risk decreases with the level of risk.

Table 1.6: Partitioned Fixed Effects Models

	Dependent Variable			
	(5) *Wage*	(6) ln(*Wage*)	(7) *Wage*	(8) ln(*Wage*)
Lowest Quartile of Fatality Rate*Fatality Rate	5.2370 ***	0.1323 ***	2.4088 **	0.0604 **
	[1.1929]	[0.0431]	[1.1998]	[0.0268]
Middle Quartiles of Fatality Rate*Fatality Rate	3.9870 ***	0.1083 ***	1.4418 *	0.0368 ***
	[0.6539]	[0.0153]	[0.7607]	[0.0130]
Highest Quartile of Fatality Rate*Fatality Rate	2.5441 ***	0.0746 ***	0.7668	0.0263 ***
	[0.4186]	[0.0097]	[0.4902]	[0.0097]
Worker Effects	Y	Y	Y	Y
Worker-Firm Effects	N	N	Y	Y
Constant	27.462 ***	3.042 ***	37.024 ***	3.306 ***
N Obs.	1351	1349	1188	1186
N Clusters	133	133	184	184
R^2	0.457	0.621	0.521	0.762
P-value of F-Test, Lowest=Middle=Highest	0.0015	0.0007	0.2235	0.2730

* Significant at the .10 level, ** significant at the .05 level, *** significant at the .01 level. † All models are weighted by the length of the job spell, measured in days. All models include year effects. Standard errors are clustered at the worker level in Models 5 and 6 and at the worker-firm level in Models 7 and 8.

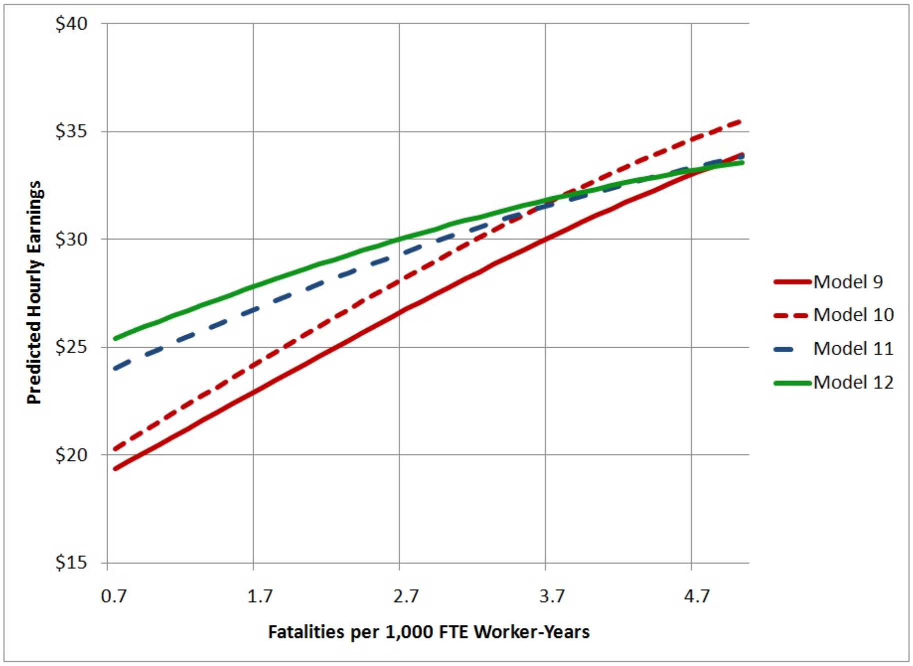
Table 1.7: Fixed Effects Models

	Dependent Variable			
	(9) ln($Wage$)	(10) ln($Wage$)	(11) ln($Wage$)	(12) ln($Wage$)
Fatality Risk	0.2156 ***	0.2277 ***	0.1393 ***	0.1199 ***
	[0.0375]	[0.0346]	[0.0412]	[0.0367]
Fatality Risk Sq.	-0.0168 ***	-0.0197 ***	-0.0121 **	-0.0113 ***
	[0.0053]	[0.0045]	[0.0048]	[0.0041]
Fatality Risk Cu.	0.0004 *	0.0005 ***	0.0003 **	0.0003 **
	[0.0002]	[0.0002]	[0.0002]	[0.0001]
Constant	2.110 ***	2.861 ***	3.088 ***	2.704 ***
Worker Effects	N	Y	N	Y
Firm Effects	N	N	Y	Y
Worker-Firm Effects	N	N	N	Y
N Obs.	1333	1349	1186	1186
N Clusters	131	133	184	184
R^2	0.232	0.633	0.631	0.766

* Significant at the .10 level, ** significant at the .05 level, *** significant at the .01 level. † Model 9 includes fishing experience, fishing experience squared, education, race, and marital status. Standard errors are clustered at the worker level in Models 9 and 10, and at the worker-firm level in Models 11 and 12. All models are weighted by the length of the job spell, measured in days. All models include year effects.

To examine the nature of the nonlinearity further, Tables 1.7 and 1.9 present models that include polynomials of the fatality rate. Models 9-12 include third order polynomials of fatality rate. The results suggest the wages are highly nonlinear functions of the fatality rate, as the coefficients on the second and third order polynomial terms are relatively large in magnitude and significant in all four specifications. The large negative coefficient on the squared fatality rate term implies concavity in the predicted wage function, as shown in Figure 1.5.

Figure 1.5: Predicted Hourly Earnings vs. Fatality Rate



Model 9 is estimated using the data cross-sectionally and includes controls for fishing experience, squared experience, education, race, year, and marital status. This model is similar to the general model used by nearly every compensating differential estimate in the literature, and the possibility of omitted variable bias must be considered. In Model 10, individual effects, which control for unobserved heterogeneity in individual productivity, are also included. This source of bias appears to be quite small in these data, as the parameter estimates in Models 9 and 10 are quite similar. All other studies in the literature use models similar to either Model 9 or Model 10. The addition of firm effects in Model 11 is therefore new; inclusion of firm effects has a substantial impact on the estimated compensating differential, suggesting that the omission of controls for unobserved firm heterogeneity in Models 9 and 10 causes very large bias in the parameter estimates. In addition to unobserved firm effects, Model 12 adds worker-firm match effects, which control for heterogeneity in productivity that could arise if workers have firm-specific skills. In Model 12, all sources of static omitted variable bias are removed. The results suggest that these biases are very large relative to the unbiased estimate of the compensating differential.

There are several reasons why the effects of firm characteristics are especially large in these data. First, there are no observed characteristics of firms included in the model, so firm effects include characteristics that are commonly observed in most data. Perhaps more importantly is the seasonal change in the composition of vessels in these data.[17] Vessel capital is not completely fungible, and so vessels operate for certain seasons, but may not operate year-round. Consequently a highly productive firm in the summer may be very different from a highly productive firm in the winter. Seasonality of the estimated firm effects suggests that the composition of firms across the year contributes to the relatively large impact of firm characteristics on estimates of the compensating differential.[18]

[17]To be clear, I refer to each vessel as a firm, although in some atypical cases several vessels share the same owner. The firm effects included in the analysis are comparable to establishment effects.

[18]I am grateful to Caroline Hoxby for pointing this out.

Figure 1.6: Predicted Marginal VSL vs. Fatality Rate

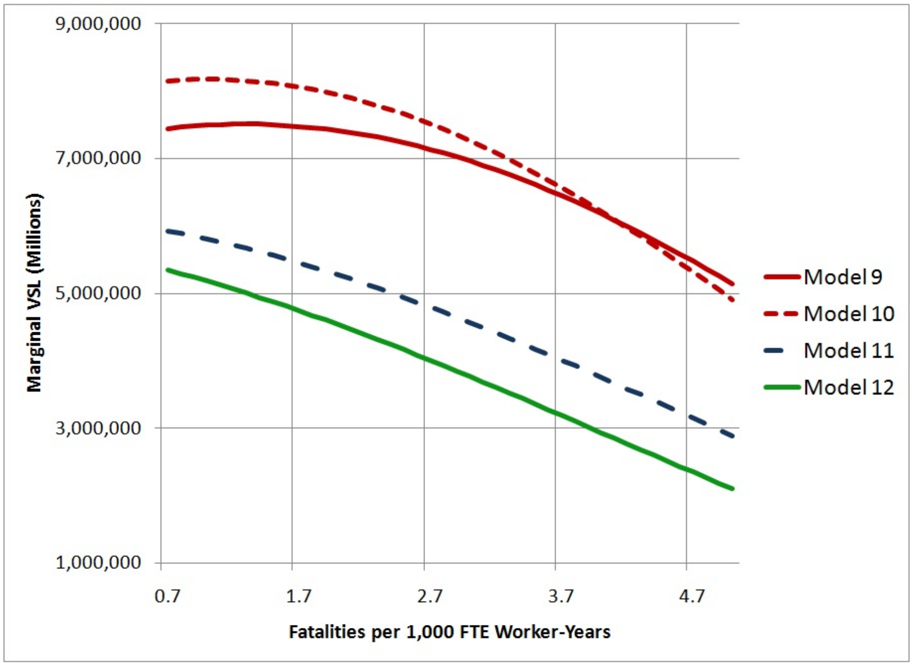


Figure 1.6 shows the marginal VSL implied by Models 9-12 as a function of the fatality rate. The estimated VSL is decreasing sharply in the fatality rate in all models. The estimated VSL in full fixed effects specification decreases from about \$6.17 million at a fatality rate of 70 to about \$2.43 million at a fatality rate of 500 deaths per 100,000 FTE worker-years, a reduction of over 60%.

Table 1.8: Box-Cox Transformation Estimates

Dependent Variable: Hourly Wage		
Lambda	-0.019	-0.063
	[0.022]	[0.021]
Fatality Rate	0.064	-0.389
Fatality Rate Sq.		0.612
Fatality Rate Cu.		-0.208
Constant	3.444	2.986
N Obs.	1349	1349
Restricted Log-Likelihood, Lambda=0	-6515.2	-6494.1
LR Statistic, χ_1^2	0.76	8.59
Restricted Log-Likelihood, Lambda=1	-7464.6	-7449.9
LR Statistic, χ_1^2	1899.5	1920.1

Table 1.8 shows the parameter estimates from a Box-Cox transformation model.

$$\frac{w_{it}^{\lambda} - 1}{\lambda} = \alpha_{ij} + \gamma_t + \beta_5 f_t + \beta_6 f_t^2 + \beta_7 f_t^3 + u_{ijt}$$

where α_{ij} is the coefficient for worker i at firm j and f_{it} is the fatality rate. In a first-order model of fatality rate, the maximum likelihood estimate of the transformation parameter, lambda, is -0.019, and is statistically indistinguishable from zero. The hypothesis that lambda=0, which implies that the best fitting model is log-linear in wages, cannot be rejected. The hypothesis that lambda=1, suggesting that a linear model in wages provides the best fit, is strongly rejected. This finding is consistent with estimates in the literature by Viscusi and Moore (1988). The third-order model in fatality rate has a slightly lower estimated lambda, -0.063, and the null hypothesis that lambda=0 is rejected at the 1% significance level. Still, the estimates suggest that the log-linear wage model has a much better fit than the linear model.

Table 1.9: Fixed Effects Models, Specification Sensitivity

	Dependent Variable			
	(13) *Wage*	(14) *Wage*	(15) ln(*Wage*)	(16) ln(*Wage*)
Fatality Risk	8.2780 ***	5.2007 **	0.1140 **	0.1398
	[1.4899]	[2.2269]	[0.0507]	[0.0893]
Fatality Risk Sq.	-0.7317 ***	-0.5355 **	-.0116 **	-0.0179
	[0.1924]	[0.2552]	[0.0055]	[0.0189]
Fatality Risk Cu.	0.0182 ***	0.0149 *	0.0003 *	0.0008
	[0.0062]	[0.0078]	[0.0002]	[0.0014]
Fatality Risk 4th				0.0000
				[0.0000]
Constant	21.156 ***	30.384 ***	3.752 ***	3.728 ***
Worker Effects	Y	Y	Y	Y
Firm Effects	N	Y	Y	Y
Worker-Firm Effects	N	Y	Y	Y
Weighted	Y	Y	N	Y
N Obs.	1351	1188	1186	1186
N Clusters	133	184	184	184
R^2	0.465	0.525	0.625	0.625

* Significant at the .10 level, ** significant at the .05 level, *** significant at the .01 level. † Standard errors are clustered at the worker level in Model 13, and at the worker-firm level in Models 14, 15, and 16. Models 13, 14, and 16 are weighted by the length of the job spell, measured in days. All models include year effects.

Table 1.9 shows estimates for variations on Models 9 and 12 to verify the robustness of the results to model specification. Models 13 and 14 are linear in wages with worker effects and worker-firm effects, respectively. The coefficients in both models have qualitatively similar implications regarding the effect of risk levels on the marginal VSL. Quantitatively, the estimated VSL is somewhat larger based on the results from Model 14, and decreases from about $9.0 million at a fatality rate of 70 to about $2.0 million at a fatality rate of 500 per 100,000 FTE worker-years. Model 15 is similar to Model 12, except that weights are removed. The effect of weights on the coefficients is negligible, although removing them increases the standard errors slightly, and decreases the fit. In Model 16 a fourth order measure of the fatality rate is added to Model 12. The effect is a large increase in the standard errors, and a reduction in the overall fit of the model. The preferred model specification, considering these robustness checks and the Box-Cox estimation results, is Model 12.

1.6 A Dynamic Model of Labor Supply with Specific Skills

Empirically, the earnings of fishing deckhands increase significantly with experience. This suggests that both wages and the accumulation of specific skills that affect future earnings are relevant for labor supply decisions. Reduced-form approaches to modeling the dynamic effects of skill accumulation are problematic because the inclusion of worker-effects causes multicollinearity with experience, and the omission of worker-effects causes bias. Thus, a dynamic model is used to account for the intertemporal consequences of labor supply decisions. This allows one to separately model the effects of experience on the terms of labor contracts and the effects of individual heterogeneity on labor supply decisions. The model is written to capture the salient features of commercial fishing jobs in BSAI fisheries.

The economic model of dynamic labor supply describes how heterogeneous workers choose to supply their labor during finite careers. Heterogeneity of workers is characterized by differences in individual utility function parameters and differences in preferences for fatality risk. Workers are assumed to be completely attached to the labor market, and choose in each period between a fishing job or an alternative job. Fishing jobs require specific skills, so experience in fishing is not perfectly substitutable for general work experience. The labor supply decision made in each period thus has dynamic implications for future labor market opportunities through the accumulation of specific skills.

The jobs differ in hourly earnings, fatality risk, and uncertainty. Fishing jobs have seasonally-dependent fatality risk, as well as uncertainty about total earnings and hours worked. The alternative offer has a fixed level of fatality risk, and no wage uncertainty. In each period t worker i chooses to work in either fishing or the alternative job. The alternative job pays a fixed hourly wage, w_{itm}, which is worker-specific and may depend on general work experience. Fishing jobs pay revenue-sharing contracts that result in a stochastic hourly earnings, r_{fit}. The revenue-sharing offer is revealed to the worker in advance of his decision. In each period the worker forms an expectation about the fishing vessel revenue and the number of hours of work required conditional on the revealed contract, which allows him to predict the distribution of his individual earnings and hours. The probability of individual i surviving period t is $\tilde{\delta}_{ft}$ for fishermen and $\overline{\delta}_m$, a fixed constant, for workers who choose the alternative.

The endowment of time in each season is denoted T. If workers choose to supply labor to a fishing vessel they have no choice over the bounded stochastic amount of hours they will work, denoted $(T - L_{fit})$. If workers choose the alternative job they have a choice over leisure time L_{mit} and hours spent working $(T - L_{mit})$.

A general utility function, which depends on consumption, leisure, and survival, is written as $g_i(\delta|\theta_i)U_i(C_{it}, L_{it})$, where utility over consumption, C_{it}, and leisure, L_{it}, can take any general form U_i in which the parameters are individual-specific. The effect of fatal risk on utility is assumed to be separable, and represented by the function g_i, with argument δ equal to the probability of survival. For example, a

Cobb-Douglas representation of utility could be written as $\delta_{it}^{\theta_i} C_{it}^{\alpha_{1i}} L_{it}^{\alpha_{2i}}$. The sequential problem corresponding to this dynamic model is:

$$\max_{(F_{it},C_{it},L_{it})} \sum_{t=1}^{T} F_{it}\beta^t\tilde{\delta}_{ft} \int_Z g_i(\tilde{\delta}_{ft}|\theta_i)U_i(C_{it}, L_{it})Q(z, dz) + (1 - F_{it+1})\beta^t\overline{\delta}_m U_i(C_{it}, L_{it}) \tag{1.1}$$

subject to:

$$C_{it} = F_{it}(T - L_{fit})r_{fit} + (1 - F_{it})w_{mit}(T - L_{mit})$$

The state variable is lifetime fishing experience, denoted e_{it}, which affects the hourly earnings of workers when they work in fishing. Control variables are: C_{it} is consumption in season t, L_{it} is leisure in season t, and F_{it} is an indicator variable taking value 1 when working in fishing in season t and value 0 otherwise. The state variable is governed by the equation of motion: $e_{it} = e_{it-1} + F_{it-1}$.

z is a stochastic vector with dimension equal to the number of sources of uncertainty in fishing, and with joint distribution Q. The elements of z include vessel revenue and hours worked, and the joint distribution Q is estimated conditional on the season, species, and revenue-sharing contract. In the budget constraint, r_{fit} can be thought of heuristically as an hourly wage, but is determined by vessel earnings and hours, as the expectation over the conditional distribution Q. Since $g_i(\tilde{\delta}_{ft}|\theta_i)$ and does not depend on the elements of z, it can be taken outside of the expectation.

The key parameters of interest are the elements of the vector θ_{it}, which characterize the effect of fatal risk on utility given function $g(\cdot)$. Normalizing the range of the function $g_i(\tilde{\delta}_{ft}|\theta_i)$ to be $[0,1]$[19], such that $g_i(\bar{\delta}_m) = 1$ and $g_i(0) = 0$, one can think of $g_i(\tilde{\delta}_{ft}|\theta_i)$ as the factor by which the utility of a fishing job, $U_i(C_{it}, L_{it})$, is discounted due to the fatality risk.

The single-period problem faced by the worker is to choose the branch of the optimization problem that maximizes the lifetime sequential problem. The probability of a worker choosing to supply his labor to a fishing vessel can be expressed as:

$$\Pr\left[F_{it} = 1|e_{it}, z\right] = \Pr\left[\beta\tilde{\delta}_{ft} g_i(\tilde{\delta}_{ft}|\theta_i)\mathbb{E}\left[J_t^f(e_{it}, z)\right] > \beta\bar{\delta}_m \mathbb{E}\left[J_t^m(e_{it}, z)\right]\right] \tag{1.2}$$

where J_t^f and J_t^m are, respectively, the suprema of the value function conditional on choosing fishing and conditional on choosing the alternative job, in period t. That is, either J_t^f or J_t^m coincides with the optimal policy, and the other choice deviates from the optimal policy in period t only.

Define:

$$\Gamma_{it} = \frac{\mathbb{E}\left[J_t^m(e_{it}, z)\right]}{\mathbb{E}\left[J_t^f(e_{it}, z)\right]}\left(\frac{\bar{\delta}_m}{\tilde{\delta}_{ft}}\right)$$

The probability of accepting a fishing job can then be written as:

$$\Pr\left[F_{it} = 1|e_{it}, z\right] = \Pr\left[g_i(\tilde{\delta}_{ft}|\theta_i) - \Gamma_{it} - \varepsilon_{it} > 0\right] \tag{1.3}$$

Denoting by Φ_i the cumulative distribution of ε_i,

$$\Pr\left[F_{it} = 1|e_{it}, z\right] = 1 - \Phi_i\left[g_i(\tilde{\delta}_{ft}|\theta_i) - \Gamma_{it}\right] \tag{1.4}$$

[19]Since $\tilde{\delta}_{ft}$ is a probability, the domain of g is also $[0,1]$

The expression for the probability of accepting the alternative job is similar, and the individual likelihood function is:

$$L_i\left[\theta_{it}\right] = \prod_{t=1}^{T}\left[F_{it}\left[1-\Phi_i\left[g_i(\tilde{\delta}_{ft}|\theta_i)-\Gamma_{it}\right]\right]+(1-F_{it})\left[\Phi_i\left[\Gamma_{it}-g_i(\tilde{\delta}_{ft}|\theta_i)\right]\right]\right] \tag{1.5}$$

The parameters are estimated using a nested fixed-point algorithm. The outer algorithm uses a Newton-Raphson search over the parameter space to maximize the likelihood function, while the inner algorithm solves the dynamic programming problem to estimate the conditional suprema of the value function, $\mathbb{E}\left[J_t^m\right]$ and $\mathbb{E}\left[J_t^f\right]$, for each iteration of the parameter search.

1.6.1 Identification

This section abstracts from estimation to discuss what is identified in the structural model. The choice to accept the fishing job in period t implies

$$g_i(\tilde{\delta}_{ft}|\theta_i) > \frac{\overline{\delta}_m\mathbb{E}\left[J_t^m\left(e_{it},z\right)\right]}{\tilde{\delta}_{ft}\mathbb{E}\left[J_t^f\left(e_{it},z\right)\right]} \tag{1.6}$$

The right hand side of this equation contains only observable or estimable objects, and identifies the sharp lower bound of $g_i(\tilde{\delta}_{ft}|\theta_i)$. Note that since $g_i(\tilde{\delta}_{ft}|\theta_i)$ is a type of discount factor, the implied compensating differential is proportional to $1-g_i(\tilde{\delta}_{ft}|\theta_i)$, so a lower bound on $g_i(\tilde{\delta}_{ft}|\theta_i)$ corresponds to an upper bound on the compensating differential and VSL. Conversely, in periods during which the alternative job is chosen, the same right hand side is an estimate of the sharp upper bound on $g_i(\tilde{\delta}_{ft}|\theta_i)$. That is, the acceptance of fatal risk from the fishing job identifies an upper bound on the compensating differential, while the rejection of a wage-risk fishing job offer identifies a lower bound on the compensating differential. Since the equilibrium price of fatality risk is determined by the preferences of the marginal worker, the preferences of each individual in the sample are identified by the upper and lower bounds of the set of possible values of $g_i(\tilde{\delta}_{ft}|\theta_i)$ that are consistent with their observed choices, and need not be point-identified for all workers.

An order statistic approach to estimating the bounds on preferences suggests that the highest value of the lower bound and the lowest value of the upper bound identify the set of possible values of $g_i(\tilde{\delta}_{ft}|\theta_i)$. In this case, the precision of estimates depends on the magnitude of variation in wages and fatality risk, and on the incidence of switching in the data. Variation in wages and fatality risk for workers who choose fishing jobs for multiple seasons will tend to shrink the identified set, all else equal. However, the set of values of $g_i(\tilde{\delta}_{ft}|\theta_i)$ need not shrink to a function asymptotically in t, and is generally identified as a correspondence. Intuitively, this is because the market equilibrium compensating differential reflects the preferences of the marginal worker, and need not reflect the preferences of all workers. In a panel setting, if the preferences of a worker are always very different than the preferences of the marginal worker, the identified set will tend to be larger for that individual.

1.6.2 Modelling Ex Ante Expectations

An important distinction to make is that between the ex ante expectations of workers, which guide labor market decisions, and the ex post outcomes observed in the survey data. The first step in estimating the structural parameters is to estimate workers' ex ante expectations about revenue, hours, labor contracts, and fatality risk. This is done using ex post data to estimate the distribution of conditional outcomes, and using the estimated distributions to describe the ex ante expectations of workers in the dynamic labor supply model. Since total earnings, hours worked per season, and revenue-sharing contracts are likely to depend on one another, expectations about these outcomes are estimated jointly using a mixture model. A mixing distribution is used to capture unobserved differences in the data generating processes, such as vessel captain effects. The return to accumulating job-specific fishing experience is described by an estimated Markov probability transition matrix between revenue-sharing contracts. All of the distributions are estimated separately for each season of the year and each species. Historical weather data are used as instrumental variables for fatality risk, so risk is also based on ex ante observable information in the model.

Ex ante expectations about vessel revenue, hours, and labor contracts

Workers' ability to form expectations about vessel revenue and hours worked per season varies across seasons and species. For example, a worker in a rationalized Red King Crab fishery knows exactly the quantity of crab that will be caught, since it is determined by tradeable quotas and quotas are sufficiently valuable that they are not left unused, and the output price per unit is negotiated in advance of the season. The primary source of uncertainty for this worker is the number of hours that it will take to earn the fairly predictable revenue. In contrast, a worker in a salmon fishery has considerably more uncertainty in the amount of revenue that will be earned, but can predict more confidently the number of hours that he will work, since he is permitted to fish for the duration of the regulated season regardless of the quantity caught.

Revenue and hours are also likely to be correlated in many fisheries. For example, in a rationalized fishery revenue and hours may be inversely correlated if choosing a lucky spot leads to earning a fixed amount of revenue more quickly. In fisheries without quantity limits one might expect a positive correlation to arise if a boat that has found a good location tends to fish more hours per day. The dependence between vessel revenue and hours worked may be unique for different seasons of the year and species.

Using season-level data from each worker on hours worked, total earnings, and revenue-sharing contracts, I calculate the revenue of the vessel and the number of hours worked. Since individual labor contracts may change across seasons for unobservable reasons, and since decisions over hours worked are made at the vessel-level by the captain, I jointly estimate vessel revenue and hours, rather than individual earnings and hours. Since workers with higher revenue-sharing contracts presumably generate more value-added for the vessel, the distribution of hours and vessel earnings is estimated jointly with individual revenue-sharing contracts. The joint distribution is estimated as a mixture of Gaussian functions using maximum likelihood.

Once a specific revenue-sharing contract is offered to the worker, expectations about revenue and hours worked are estimated by integrating the joint distribution of revenue, hours, and contracts over the observed revenue-sharing level in the revealed contract. Expected utility is then calculated over the joint distribution of vessel earnings and hours conditional on the revealed contract, season, and species.

Ex ante expectations about labor contracts

While vessel revenue and hours are exogenous stochastic variables, workers' labor contracts in each period depend on the state variable, e_{it}. One can estimate the dependence of revenue sharing levels on past fishing experience as a Markovian stochastic process, and use this process as a model of the ex ante expectations held by workers in each period about future levels of revenue-sharing. The process is represented by a transition probability matrix, which workers use to form expectations over value functions in future periods.

Specifically, if there are k possible revenue-sharing contracts, the transition matrix of interest, denoted P, is a $k \times k$ dimensional probability transition matrix, with elements $P_{k,\ell}$ equal to the probability that a worker will move from revenue sharing contract k to contract ℓ with one additional year of experience. If R is the $k \times 1$ vector of the discretized levels of revenue sharing, then denote by $(I = k)P^{(e_{it})}R$ the kth element of $P^{(e_{it})}R$, which is the expected revenue sharing level for a worker who was last offered contract k if he works an additional e_{it} periods in fishing.

There are several potential approaches to estimating the transition probabilities in P. With sufficient data, one could simply calculate the probability of observing each transition in the data. However, since this matrix is estimated separately for each quarter of the year and each species, there are insufficient data to estimate this matrix unless a very course vector R is used. Instead, P is estimated using both reported lifetime fishing experience and changes to fishing experience observed during the panel. That is, the estimated elements $P_{k,\ell}$ of the matrix P are those that are most likely to give rise to the observed distribution of experience and revenue-sharing contracts. This is done by solving the minimization problem:

$$\min_P \sum_{i,t} (y_{it} - (I = 1)P^{e_{it}}R)^2 \tag{1.7}$$

$$s.t. \sum_k (P_k) = 1 \forall k$$

where y is the $t \times 1$ vector of observed values of revenue-sharing for person i, e_i is the $t \times 1$ vector of experience for person i, and $(I = 1)P^{e_i}R$ indicates the first element of the resulting $k \times 1$ vector. The first element is taken on the assumption that workers with no past experience begin with revenue sharing contracts in the first bin of the discretized revenue-sharing grid. Solving this problem minimizes the sum of squared residuals between observed revenue-sharing levels and the levels predicted by the transition matrix P.

Using this estimation approach, if there are no restrictions on the probabilities there are $k \times (k-1)$ free parameters to estimate. Imposing that restriction that there are no demotions restricts the lower diagonal half of P to be zeros, leaving $(k^2-k)/2+k-1$ free parameters. Imposing the additional restriction that promotions in each period cannot be greater than one grid point reduces the number of free parameters to $2k-2$. In the results reported here, P is restricted to be an upper triangular matrix.

1.7 Results

1.7.1 Structural Results

To estimate the structural model, functional forms for U and g must be specified. Models shown in the results include utility functions that are linear in consumption, Cobb-Douglas, and constant elasticity of substitution (CES), and g is a polynomial function of the level of risk. The error is assumed to be distributed normally, $\varepsilon_{it} \sim N(0, \sigma_i^2)$.

Table 1.10: Functional Form Assumptions in Structural Models

Model Number	$U_i(C_i, L_i)$	$g_i(\tilde{\delta}_{ft} \mid \theta_i)$
S1	C_i	θ_{0i}
S2	$C_i^{\alpha_i} L_i^{1-\alpha_i}$	θ_{0i}
S3	$C_i^{\alpha_i} L_i^{1-\alpha_i}$	$\theta_{0i} + \theta_{1i}\tilde{\delta}_{ft}$
S4	$C_i^{\alpha_i} L_i^{1-\alpha_i}$	$\theta_{0i} + \theta_{1i}\tilde{\delta}_{ft} + \theta_{2i}\tilde{\delta}_{ft}^2$
S5	$[\alpha_i C_i^{\rho_i} + (1-\alpha_i) L_i^{\rho_i}]^{(1/\rho_i)}$	$\theta_{0i} + \theta_{1i}\tilde{\delta}_{ft} + \theta_{2i}\tilde{\delta}_{ft}^2$

The periods in the model are calendar quarters from 2003-2009. The panel is unbalanced, with between 24 and 28 quarters of data per individual. Fatality rates are average quarterly fatality rates matched to the timing of the species seasons, so there is some variation in risk across species in each quarter due to differences in the average timing of fishing trips by species. The wage in the alternative job is imputed for each worker using data from the CPS, as described in Appendix 1.[20]

[20] For robustness, I also estimate the structural model using wages from non-fishing employment

Table 1.11: Structural Parameter Estimates, Set Estimated θ_i

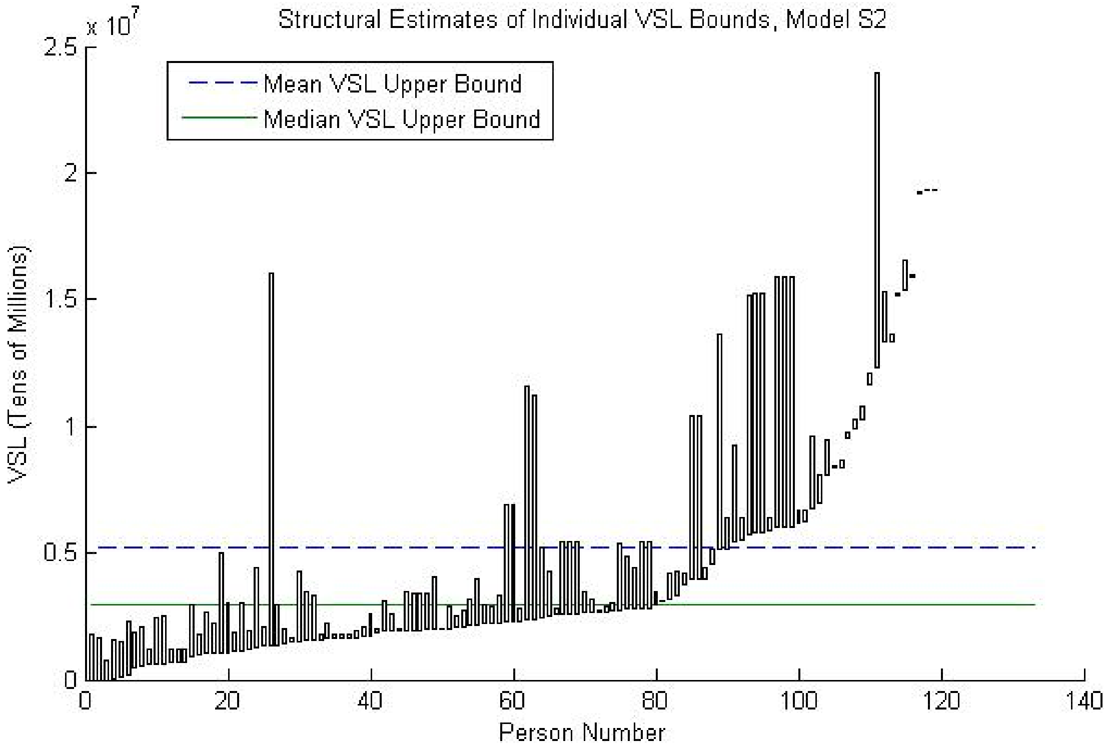
Model		(S1)	(S2)
Utility Function		Linear	CD
θ_i Upper Bound	Mean	0.823	0.742
	Median	0.838	0.824
	S.D.	0.074	0.206
θ_i Lower Bound	Mean	0.818	0.728
	Median	0.834	0.834
	S.D.	0.111	0.246
α_i	Mean	$1^\dagger$	0.557
	Median	$1^\dagger$	0.739
	S.D.	$0^\dagger$	0.394
β		$0.98^\dagger$	$0.98^\dagger$
σ^2		$0.05^\dagger$	$0.05^\dagger$
Implied VSL_i Upper Bound (Millions)	Mean	3.61	5.58
	Median	2.80	2.82
	S.D.	3.14	7.36
Implied VSL_i Lower Bound (Millions)	Mean	3.51	5.21
	Median	2.82	2.96
	S.D.	2.17	6.53

† Restricted parameter

The structural parameter estimates are presented in Tables 1.11 and 1.12, and Figure 1.7. Table 1.10 shows the functional forms used in each of the models estimated. Tables 1.11 and 1.12 presents maximum likelihood estimates under the specified assumptions.

reported in the survey data, and impute the wage for those who do not report one using the sample data. The estimated VSL differs by less than 5% under the alternative assumption.

Figure 1.7: Structural Estimates of Identified VSL Set, Model S2, Sorted by Lower Bound



The first two models estimate the set of values that contain the latent $\tilde{\theta}_i$. Table 1.11 presents the structural results from set-estimated models similar to those in Table 1.12. Models S1 and S2 both assume the outside option to be the imputed CPS wage, and the models differ only in the functional form of utility. In Model S1, the maximum likelihood estimate of the mean upper bound on the set containing θ_i is 0.823, while the estimated mean lower bound is 0.818. The implied upper and lower bounds on the estimated VSL are also quite close to each other, with an estimated mean lower bound of \$3.51 million and mean upper bound of \$3.61 million. Model S2 allows for more flexible heterogeneity in preferences with a Cobb-Douglas utility function, and the estimated VSL set increases to between \$5.21 million and \$5.58 million.

Table 1.12: Structural Parameter Estimates

Model		(S3)	(S4)	(S5)
θ_{0i}	Mean	0.766	0.746	0.825
	S.E.	[0.022]	[0.023]	[0.014]
	Median	0.848	0.843	0.871
θ_{1i}	Mean	-15.006	-10.311	-85.640
	S.E.	[1.338]	[0.890]	[6.412]
	Median	-12.040	-7.608	-58.338
θ_{2i}	Mean	$0^{\dagger}$	2.811	5.090
	S.E.	$0^{\dagger}$	[0.368]	[0.728]
	Median	$0^{\dagger}$	1.380	4.200
α_i	Mean	0.695	0.621	0.555
	S.E.	[0.023]	[0.026]	[0.019]
	Median	0.773	0.686	0.501
ρ_i	Mean	$0^{\dagger}$	$0^{\dagger}$	0.353
	S.E.	$0^{\dagger}$	$0^{\dagger}$	[0.019]
	Median	$0^{\dagger}$	$0^{\dagger}$	0.340
σ_i	Mean	0.200	0.234	$0.08^{\dagger}$
	S.E.	[0.019]	[0.020]	$0^{\dagger}$
	Median	0.119	0.147	$0.08^{\dagger}$
β		$0.995^{\dagger}$	$0.995^{\dagger}$	$0.995^{\dagger}$
Implied VSL_i (Millions)	Mean	\$4.905	\$4.060	\$6.750
	S.E.	[\$0.614]	[\$0.559]	[\$0.539]
	10th Pctl	\$0.356	\$0.322	\$1.452
	25th Pctl	\$1.048	\$1.194	\$3.258
	Median	\$3.559	\$3.136	\$5.397
	75th Pctl	\$5.443	\$5.044	\$7.398
	90th Pctl	\$9.888	\$7.240	\$15.243

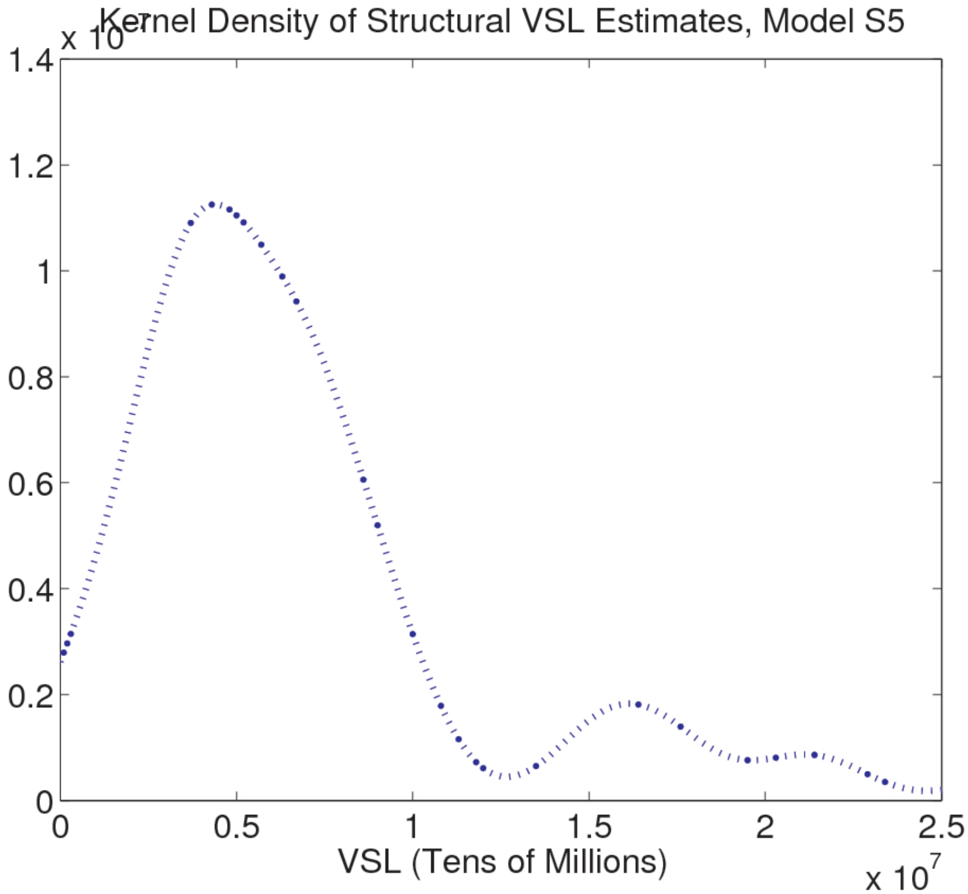
† Restricted parameter

Since the identified sets are quite small in Models S1 and S2, Models S3-S5 provide point estimates of the parameters, which computationally allows more flexibility in the functional form assumptions. In Model S3, utility is Cobb-Douglas, allowing heterogeneity in workers' preferences for consumption and leisure, and g_i is linear in fatality rate. Heterogeneity in g_i allows workers to have heterogeneous preferences for risk. The mean estimate of g_i is $0.766 - 15.006 \times R$, where R is the probability of death. The statistical value of life is estimated by applying the estimated discount factor for fatality risk, g_i, to the stream of earnings, computing the difference between the discounted and undiscounted earnings, and dividing this difference by the cumulative probability of fatality associated with the optimal policy. The functional form in Model S3 implies a constant VSL that is independent of the level of risk, and the mean estimate of the individual VSLs is \$4.91 million, with a 95% confidence interval of \$3.68 to \$6.13.

Model S5 is the most flexible model. The use of a CES utility function relaxes the assumption implied by the Cobb-Douglas model that $\rho_i = 0$. Model S5 also includes a quadratic term in g_i, which allows the VSL to change with the level of risk. In this model, the mean of g_i is estimated to be $0.825 - 85.64 \times R + 5.09 \times R^2$. The implied VSL is a decreasing function of the level of risk, due to the positive coefficient on the quadratic term.

In comparing these estimates to others, it should be noted that reduced-form estimates of compensating wage differentials rely upon the acceptance of risk for identification. Therefore, in set-estimated models, the structural estimate that is most comparable to a reduced-form estimate is the mean upper bound rather than the mean lower bound, which is instead identified by the rejection of risk and acceptance of the outside option.

Figure 1.8: Kernel Density of VSL, Model S5



The structural results also provide evidence on the degree of heterogeneity in preferences for risk among workers in the sample. The sets of VSLs corresponding to the estimated θ_i sets for each individual in the sample are shown in Figure 1.7. The degree of heterogeneity is very large, and the standard deviation of the estimated distribution of upper bounds on the VSL is \$7.36 million in Model S2; note that this estimate is larger than the sample mean. More than half of the sample has estimated VSLs below \$3 million, while over 10% of respondents have high VSLs above \$10 million. In Model S5, the 10th percentile of individual VSLs is \$2.23 million, while the 90th is nearly \$16 million. Figure 1.8 displays the kernel density of the distribution of estimates from Model S5 of individual VSLs.

1.7.2 The Identification of Preferences for Fatal Risk

Hwang, Mortensen, and Reed (1998) examine a prototypal model of hedonic wages with labor market search and heterogeneous firms. They describe how a basic dynamic job search model causes the error term in standard labor market hedonic wage models to violate the assumptions of the classical linear regression model. To demonstrate simply, consider a model in which workers are homogeneous and firms are heterogeneous. Worker i at firm j receives wage w_{ij} and fatality rate x_j. A linear hedonic regression specification of this job search model can be written as:

$$w_{ijt} = \alpha + \beta x_{jt} + v_{ij} + \epsilon_{ijt}$$

where β is the true marginal willingness to pay (MWP) for safety (or willingness to accept fatal risk,) and the error term includes a classical component, ϵ_{ijt}, and a stationary component, v_{ij}, which captures the unobserved heterogeneity in firm productivity. The hedonic model provides biased estimates of the true marginal willingness to pay, β.

$$\mathbb{E}(\beta) = \beta + \mathbb{E}[(X'X)^{-1}X'V]$$

where X is a matrix of ones and x_{jt}s, and V is a vector of v_{ij}s. The bias,

$$Bias = (X'X)^{-1}X'V$$

is positive under the assumption that firms that are more productive generally are also better able to produce safety. As Hwang et al point out, in general fixed effects cannot be used to correct this bias because a firm-effect would be perfectly collinear with the fatality rate, x_j.

However, in the current setting, changes in weather conditions cause exogenous changes in firms' technology for producing safety. As a result, the fatality rate enters the hedonic model as x_{tj}, permitting the inclusion of either a firm-effect v_j or a worker-firm effect v_{ij} without causing collinearity. In this case v_{ij} enters the model as an intercept term rather than an error component, and β is an unbiased estimate of the MWP.

It is unclear ex ante what shape one should expect the MWP to have in wage-risk space. Several studies[21] have estimated the wage-risk relationship to be a concave function of risk. The concavity estimated in these studies is significant enough that in order for it to be fully attributable to bias, the bias would have to be extremely large and correlated with the level of risk. The estimates in this study confirm that both the equilibrium wage-risk locus and workers' marginal willingness to pay for safety are decreasing in the level of fatal risk, implying that aversion to marginal increases in risk falls as the level of risk increases. To be clear, the finding that the MWP decreases as the level of risk increases does not imply that safety is not a normal good; rather, the finding implies that indifference curves in wage-risk space become flatter as the level of risk increases. The results of this are not inconsistent with results in the literature that imply that the VSL increases as wages increase. Kniesner, Viscusi, and Ziliak (2008) find this result using panel data to control for

[21] Viscusi (1981), Olson (1981), Dorsey and Walzer (1983), and Leigh and Folsum (1984)

individual heterogeneity, although their estimates contradict most other estimates. The combination of these findings suggests that indifference curves become steeper as wages increase, all else equal, and flatten as the level of fatality risk increases, all else equal.

1.7.3 (Some) Limitations of Results

Although there may be compensating wage differentials for many job characteristics (such as the potentially undesirable characteristic of leaving one's family to work at sea,) most of these characteristics do not change between seasons. The primary between-season difference in non-wage characteristics, controlling for observable changes, is the weather. It is plausible that differences in weather conditions across seasons cause wage differentials that compensate for both fatality risk and the general discomfort of working conditions. Since weather conditions are used as instruments for fatality risk, it is not possible to separately identify compensating differentials that may be due to changes in the weather itself, as opposed to changes in fatal risk that accompany seasonal weather change. This is due in large part to the high correlation between weather characteristics, making it impossible to include weather characteristics that cause discomfort without directly affecting the fatality rate. Attributing the estimated compensating differential entirely to fatality risk may overstate the implied VSL. Schmidt and Zimmerman (1991), however, found a wrong-sided slightly negative wage effect for working in unpleasant weather conditions. Given this empirical evidence, and the large magnitudes of fatal risk in this study, the assumption that fatality risk is responsible for a very large share of the estimated compensating differential is plausible.

Another potential source of estimation bias is due to the failure to control for differences across seasons in the nonfatal injury rate. To the extent that the rate of non-fatal injuries is positively correlated with the rate of fatal accidents, this omission may bias estimates upward. The publicly available data on nonfatal injuries are insufficient to control for this variation. However, there are limitations to the ability to control for nonfatal injuries properly in any study of compensating differentials. One reason is that it is usually not possible to accurately characterize the costs of being injured, which is necessary since unlike fatal injuries, nonfatal injuries are not all of equal severity. Consequently, estimates in the literature of the compensating differential for nonfatal risk are very imprecise. In addition, the rate of nonfatal injury is usually highly correlated with the rate of fatal injury, so including both variables causes collinearity. The coefficient on one of the two measures of risk is often insignificant when both measures are included. Both Brown (1980) and Kniesner, Viscusi, and Woock (2010) exclude nonfatal injury risk in their estimations, so there is no empirical US labor market evidence on how this exclusion might affect longitudinal estimates.

The measurement of fatality risk is a chronic problem in the estimation of compensating differentials. While the individual-level data used in this study are far more accurate than aggregated industry-level data, there are still unobservable differences in the latent fatality risk that could vary across workers and firms.

1.8 Policy Implications

There appears to be substantial individual heterogeneity in the estimated statistical value of life. The structural estimate of the upper bound on the individual VSL ranges from less than $1 million to well over $20 million, depending on the individual. The preferred mean estimate of the upper bound on VSL is about $6.75 million in the structural model, and about $6.17 million in reduced-form Model 12 at a risk level of 70 deaths per 100,000 worker-years.

Detailed information regarding the way fatality risk affects workers' marginal preferences for risk is of use to many policymakers. The compensating wage differential for fatality risk in labor markets is frequently used to guide spending on policies that affect mortality. However, mortality rates in labor markets are often quite different from mortality rates in policy options. For example, the average US manufacturing fatality rate is 3.9 deaths per 100,000 FTE worker-years, while the mortality rate for US men attributable to smoking is about 384 per 100,000 person-years. As the above results have suggested, the marginal willingness to accept a unit of risk is dependent on the level of risk. Consequently, using VSL estimates from manufacturing workers to inform policy that affects spending to reduce smoking mortalities, for example, could lead to conclusions that do not represent the preferences of affected populations for fatality risk. An approach that better reflects the preferences of the individuals accepting risk would require estimating the compensating differential of the population at the appropriate level of risk. Since this is not always possible, a second best approach is to learn about the shape of preferences for risk in the population, which provides information that can be used to more accurately extrapolate VSL estimates to levels of risk suitable for policy analysis. The graphs in Figure 1.6 and regression results from Model 12 in Table 1.7 can be used to estimate proportional changes in marginal VSL when the level of risk faced a population changes.

1.9 Conclusion

Using new survey data from commercial fishing deckhands in the Bering Sea fisheries of Alaska and new disaggregate data on workplace fatalities, I estimate the compensating differential for fatality risk by exploiting seasonal variation in hourly wages and weather conditions, which cause variation in fatality risk. This unique source of within-worker-firm variation across brief job spells allows for fixed effects estimation that removes bias caused by unobserved heterogeneity in worker and firm productivity and unobserved match effects, which was previously not possible with available labor market data. The exogeneity of weather changes, and the consequent changes in fatality rates and hourly earnings, alleviates concerns about bias caused by endogenous switching of jobs in other panel studies. These econometric advances address the majority of the remaining concerns about estimation bias discussed in the literature. Moreover, by controlling for worker and firm heterogeneity, which is the basis for assortative matching, this approach permits estimation of workers' marginal willingness to accept fatal risk, which has never before been identified in labor markets. Estimates of the marginal willingness to accept fatal risk, a fundamental preference of workers, suggest that aversion to marginal increases in risk decreases with the level of risk. The preference-based VSL, therefore, also decreases substantially with the level of risk.

A structural discrete-choice labor supply model is used to estimate the compensating differential, taking into account the dynamic effects of skill accumulation. The structural model suggests a compensating differential, normalized as a VSL measure, of about \$6.75 million, with substantial individual heterogeneity in preferences for risk. The results also support the hypothesis that compensating differentials are affected nonlinearly by the level of risk accepted by workers, and the price of marginal risk is decreasing the level of risk accepted. The reduced-form estimate of the VSL is about \$6.17 million at a rate of 70 fatalities per 100,000 worker-years, and decreases to about \$2.43 million when the fatality increase to 500. Estimates concur with theoretical predictions from Hwang, Mortensen, and Reed (1998) that the bias caused by omitting unobserved firm characteristics is positive and large in magnitude. The policy implications of this new information about individual preferences for fatal are risk are that VSL estimates used to guide public policy should be adjusted downward when the level of risk of a policy exceeds the risk at which the VSL was estimated. The results of this paper suggest proportional magnitudes of such adjustments that are consistent with the revealed preferences of workers for fatal risk.

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CHAPTER 2

THE LABOR-MARKET EFFECTS OF NON-COMPETE AGREEMENTS: EVIDENCE FROM THE MARKET FOR PHYSICIANS

(JOINT WORK WITH WILLIAM D. WHITE AND CAROL SIMON)

2.1 Introduction

Non-compete agreements (NCAs), also known as covenants not-to-compete, are elements of employment contracts that restrict the occupational mobility of workers post-employment. Their primary purpose is to prevent workers from exiting a firm and competing against it. There is substantial interest among legal scholars and policymakers in understanding how the merits of NCAs, which have the potential to increase productivity by fostering human capital investment, compare to the costs of restricting workers. However, there has been extremely little attention given to them by economists. This is in part a consequence of the lack of micro-level data that document the use of non-compete agreements in labor markets, but also because of the difficulty in estimating the mobility effects in light of the endogeneity of the acceptance of NCAs by workers.

There is a long history of controversy surrounding the use of non-compete agreements for physicians.[1] Proponents of their use argue that practices should be able to write flexible labor contracts, and that non-compete agreements are justified to protect practice investments in building relationships between patients and referring

[1]Di Dio (1999), Kinney (2008), and Whyte (1961)

physicians and in recruiting physicians. Critics, including organized medicine, have voiced concerns that non-compete agreements restrict patient choice, limit physician mobility, have anti-competitive effects on entry, and may restrain trade.[2] In particular, the American Medical Association (AMA) Council on Ethical and Judicial Affairs[3] takes the position that "Covenants-not-to-compete restrict competition, disrupt continuity of care, and potentially deprive the public of medical services." The council generally discourages their use, and specifically deems them as unethical if they are excessive in geographic scope or duration or if they fail to reasonably accommodate patients' choices of physicians.

This paper presents a search-theoretic model of labor markets with non-compete agreements, and uses data from the market for primary care physicians in five states to estimate the static compensating differential associated with NCAs, the effect they have on dynamic wage paths and the prices of physician services. These estimates provide information about the productivity benefits of NCAs relative to their effects on the monopsonistic power of firms in wage bargaining. We also present evidence on the relationships between variation across states in the enforceability of NCAs and variation in the mobility and geographic distribution of physicians.

The search-theoretic model frames the effects of NCAs within the context of on-the-job search. Non-competes affect local monopsony power of firms and the model accounts for this, following Pissarides (1994), in the determination of wages through bargaining. The model suggests that the static effects of non-compete agreements are to increase static wages and reduce the rate of job-to-job transitions. The reduction in mobility caused by non-compete agreements could occur either through

[2] Di Dio (1999)
[3] AMA (1998)

a reduction in the rate of job offers or an increase in the cost of job-switches. The dynamic effects of non-compete agreements are ambiguous. If non-compete agreements allow practices to invest in patient-physician relationships, or otherwise increase the rate of learning-by-doing, they may increase the slope of the career wage path. Alternatively, if the increase in monopsonistic power granted to firms affects bargaining over increases in wages over time, non-compete agreements could cause the wage path to flatten. This suggests an empirically testable hypothesis of the effect of non-compete agreements on productivity relative to their effects on bargaining power.

To empirically test the hypotheses suggested by the model, this paper uses a unique dataset from a survey, conducted between 2006-7, of 1,967 primary care and pediatric physicians across 5 states. Prior to this survey there had been no systematic data available on the use of NCAs in healthcare markets, although anecdotal evidence suggests that their use has been growing over time.[4] In the context of physicians, NCAs typically define a relevant set of services which physicians are restricted from providing within a defined geographic market area for specified period of time if the physician exits the practice.[5] As a consequence, if a physician subject to a non-compete agreement leaves their practice, they are likely to face a choice between temporarily giving up the practice of medicine, leaving their community

[4]Growing use over time would be consistent with a general shift in medical practice organization in the U.S. away from solo practice and self-employment towards group practices and employee status. As recently as 1984, Kletke, Emmons and Gillis (1996) estimate that 71% of physicians were in solo practices or two person groups, while 76% of physicians were owners or part-owners of their practices only 26% were employees. However, using somewhat different data, Liehaber and Grossman (2007) estimate that as of 2004-5, the share of physicians in solo or two-person groups was only 33% and that overall, 45% of physicians were employees, with 19% working as employees in physician owned practices, but with 36% employed by non-physician entities, while more recent data from Harris (2010) suggests that the trend toward non-physician ownership may be accelerating.

[5]Lowry (2003).

or, if permitted, paying a buyout. For example, under a non-compete agreement recently upheld in Kansas, a family physician leaving a medical group was prohibited from practicing for three years in the same county as the group unless she paid the group 25% of her earnings during this period.[6]

We find that the use of non-compete agreements in this labor market is extensive, with about 45% of all physicians in the sample subject to one. In addition, we estimate how practice ownership structure, geographic market characteristics, and state laws on the enforceability of NCAs affect the use of non-compete agreements. We estimate the compensating differential for accepting a non-compete agreement to be approximately 11% of average hourly earnings. We find evidence that the increase in practice costs associated with NCAs may cause consumers to face higher prices. Specifically, we estimate that the price of an initial visit from a privately-insured patient is about 25% higher in states with very strict enforceability of NCAs, controlling for other factors that affect supply and demand. We also find that states with very strict enforceability of NCAs have fewer physicians per capita and physicians in those states are less mobile. This suggests that physicians are aware of laws regarding NCAs, and respond to these laws by sorting into states and practices according to their preferences for occupational mobility.

The empirical literature on labor-market effects of non-compete agreements is very small, largely due to data limitations. Marx, Strumsky, and Fleming (2009) use an exogenous inadvertent change in enforcement of non-compete agreements in Michigan in 1985 and find that for inventors who produced patents the increase in the enforcement of non-compete agreements significantly reduced mobility relative

[6]Sorrel, AL (2008). For other anecdotal examples see Ligos (2000) or Wilson (2006).

to inventors in other states. To our knowledge, there has been no study on mobility effects outside of research and development industries, and no estimates at all of the wage impacts of non-compete agreements.

From a policy perspective, we are interested in documenting the prevalence of non-compete agreements as a condition of physician employment and in estimating the impact of these agreements on labor market outcomes in order to understand their implications for social welfare. As part of physicians' employment contracts, non-compete agreements are typically subject to review at the state level, and the degree to which they are enforceable varies considerably from state to state. The findings of this study provide the important new evidence to state policymakers on how legislation on non-compete agreements affect labor markets generally, and the market for primary care physicians in particular.

The paper proceeds as follows: Section 2 provides background on state oversight of the use of non-compete agreements in physician practices. Section 3 presents a theoretical search-based model of labor supply and demand with non-compete agreements. Section 4 discusses the data and our empirical estimation approaches. Section 5 presents our results. Section 6 concludes and discusses the possible implications of our findings for policy and future research.

2.2 State Oversight of Non-Compete Agreements

Non-compete agreements are primarily subject to review at the state level, and enforcement based on state case law and any applicable statutes. As of 2006, non-compete agreements in employment contracts for physicians were subject to court oversight in all 50 states. In addition, in 16 states, enforcement of these clauses was also governed by explicit statutes. Typically, courts seek to identify whether a practice has a legitimate, protectable business interest, and if so, to balance protecting this interest against any hardships imposed on patients or physicians. One major focus has been on protecting practice investments in developing a patient base from competition by departing physicians for existing and/or potential patients. In addition, courts may recognize business assets such as confidential client lists as protectable. Arguing that markets for medical services are typically local, it is not uncommon for courts to deem non-competes overly restrictive if they are excessive in geographic scope, and courts have also questioned the scope of activities covered.[7]

There has been a very recent increase in legislative and legal attention paid to the use of NCAs specifically for medical practitioners. In 2008 Massachusetts passed a law banning the use of NCAs for physicians and nurses, citing the effects that they have on medical professionals' rights to practice and on patients' rights to choose practitioners. In Tennessee the Supreme Court recently banned the use of NCAs for physicians in *Murfreesboro Medical Clinic, P.A. v. Udom*, which prompted the state legislature to enact a bill that specifically allows the use of NCAs for physicians other than emergency physicians.[8] The state legislature has subsequently proposed, but not yet passed, a bill that would expand the ban on NCAs to include all general physicians.

Popular summaries of the enforcement of non-compete agreements, such as Wil-

[7]Filipp (2009)
[8]Tenn. Code Ann. 63-1-148

son (2006), have been very broad, dividing states into three groups: 1) states in which non-competes are said to be "unenforceable" (6 of the 16 states with statutes and 1 state with case law); 2) states in which non-competes are "enforceable" (10 states with statues and 26 with case law only); and 3) 9 states in which case law is judged uncertain. In practice, however, issues of enforcement are much more nuanced then these summaries suggest.[9]

Recently, a much more careful and precise dataset was developed and used in Bishara (2011), which scored the interpretation of case law in each state along eight different dimensions, and quantified the restrictiveness of non-compete agreements in each state. Appendix 1 shows the questions and rules used in developing these data. The ratings take into account: 1) whether or not there are state statutes that govern the enforcement of non-compete agreements, and if so how strong they are; 2) how broadly defined an employer's protectable interests are; 3) the strength of the burden of proof that plaintiffs must show to enforce a non-compete; 4) whether the signing of a non-compete covenant at the inception of an employment relationship represents sufficient consideration for the support of the covenant; 5) whether continued employment provides sufficient consideration for a non-compete agreement entered into after the employment relationship has begun; 6) whether changes in the terms and conditions of employment are sufficient consideration; 7) the extent to which courts can modify overly broad covenants not-to-compete to make them enforceable; 8) and whether the covenant is enforceable if the employer terminates the relationship. These far richer data allow us to test how the labor market effects of non-compete agreements correspond to variation across states in enforceability,

[9]See Malsberger (2006) for a detailed review of the legal treatment of non-competes on a state by state basis.

and in particular dimensions of enforceability.

Table 2.1: Bishara (2011) Rating of the Restrictiveness of Non-Compete Agreements

	Question	Criteria	Weight
Q1	Is there a state statute that governs the enforceability of covenants not to compete?	10 = Yes, favors strong enforcement 5 = Yes or no, in either case neutral on enforcement 0 = Yes, statute that disfavors enforcement	10
Q2	What is an employer's protectable interest and how is that defined?	10 = Broadly defined protectable interest 5 = Balanced approach to protectable interest 0 = Strictly defined, limiting the protectable interest of the employer	10
Q3	What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?	10 = Weak burden of proof on plaintiff (employer) 5 = Balanced burden of proof on plaintiff 0 = Strong burden of proof on plaintiff	5
Q3a	Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?	10 = Yes, start of employment always sufficient to support any CNC 5 = Sometimes sufficient to support CNC 0 = Never sufficient as consideration to support CNC	5
Q3b	Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5

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	Question	Criteria	Weight
Q3c	Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5
Q4	If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenant enforceable? If so, under what circumstances will the courts allow reduction and what form of reduction will the courts permit?	10 = Judicial modification allowed, broad circumstances and restrictions to maximum enforcement allowed 5 = Blue pencil allowed, balanced circumstances and restrictions to middle ground of allowed enforcement 0 = Blue pencil or modification not allowed	10
Q8	If the employer teminates the employment relationship, is the covenant enforceable?	10 = Enforceable if employer terminates 5 = Enforceable in some circumstances 0 = Not enforceable if employer terminates	10

Focusing on the five states in our data, California, Illinois, Georgia, Pennsylvania and Texas, Bishara (2011) ranks these states as 50th, 4th, 43rd, 23rd, and 32nd, respectively, in strength of enforcement. This provides substantial variation across states within our sample, and the quantified ratings range from 31 in California to 430 in Illinois. Table 2.2 shows the ratings from Bishara (2011) for the states in our sample.

Table 2.2: Bishara (2011) Summary of State Restrictiveness of Non-Compete Agreements

	California	Georgia	Illinois	Pennsylvania	Texas
Average Total Score	31	285	430	365	350
State Rank*	50	43	4	23	32
Q1	10	30	50	50	80
Q2	10	70	70	70	80
Q3	5	25	30	20	35
Q3(a)	0	50	50	50	20
Q3(b&c)	0	50	50	25	15
Q4	0	0	90	80	60
Q8	0	60	90	70	60

Note: *Out of 51, including D.C.. 1 is the most restrictive.
Source: Bishara (2011). See Table **??** for explanation of question numbers.

2.3 Economic Model

Non-compete agreements have the potential to affect both labor supply and labor demand. In deciding whether or not to require non-compete agreements as a condition of employment, a practice's problem is to weigh anticipated benefits associated with NCAs against anticipated costs, which may depend on physician, practice and market characteristics, including the regulatory environment. On the physician side, non-compete clauses could affect both the extensive and intensive margins of labor supply. On the extensive margin, the exit barrier created by non-compete agreements should tend to reduce mobility. However, variation in the level of restrictiveness of NCAs across states could lead to sorting across states, so that states with strongly enforceable NCAs have either fewer physicians or physicians with on average weaker preferences for occupational mobility. Thus selection across states could act against a potential decrease in mobility caused by NCAs. This net effect on labor supply in a local labor market is ambiguous.

An intensive margin effect could occur if, for example, non-compete clauses affected the elasticity of labor supply. One possible mechanism through which this could occur is if the exit barrier created by a non-compete clause gave firms some monopsonistic power that allowed them to flatten the dynamic wage profile of a physician. All else equal, if learning by doing has less affect on the wage profile, the static elasticity of labor supply is likely to be smaller

2.3.1 A Labor Search Model with Non-Compete Agreements

The following is a simple labor search model that describes the theoretical effects of non-compete agreements on labor supply and labor demand.

Labor Supply with Non-Compete Agreements

Assume that there is no unemployment, so all transitions are job-to-job, requiring on-the-job search.[10] Suppose workers receive offers at the rate a, and that the fraction of firms whose offers have NCAs is λ. Since exiting a job with an NCA requires

[10]The unemployment rate for physicians is generally very low and quite stable, consistent with unemployment spells being primarily frictional and acyclic. For example, between May 2006 and May 2009 the national average unemployment rate rose from about 4.6% to 9.4%, while the unemployment rate for physicians and surgeons remained between 1.5% and 1.6% (Sources: http://www.bls.gov/oes/current/oes291069.htm and http://www.bls.gov/opub/ted/2009/jun/wk2/art02.htm).

that a worker move out of the market, it is generally more costly to move after signing an NCA. Assume that for a worker who has accepted a non-compete agreement, the cost of switching jobs is $L > 0$, while for a worker without an NCA the cost of switching jobs is zero. The Bellman equation for a worker who has an NCA, and who receives wage *omega* is:

$$V(\omega, 1) = \omega + \beta\alpha\left[\lambda \int_0^\infty \max\{V(\omega', 1) - V(\omega, 1) - L, 0\}\mathrm{d}F(\omega') + (1 - \lambda)\int_0^\infty \max\{V(\omega', 0) - V(\omega, 1) - L, 0\}\mathrm{d}F(\omega')\right]$$

where $\beta \in (0, 1)$ is the period discount factor and $F(\omega)$ is the distribution of wage offers. Similarly, for a worker without an NCA, the Bellman equation is:

$$V(\omega, 0) = \omega + \beta\alpha\left[\lambda \int_0^\infty \max\{V(\omega', 1) - V(\omega, 0), 0\}\mathrm{d}F(\omega') + (1 - \lambda)\int_0^\infty \max\{V(\omega', 0) - V(\omega, 0), 0\}\mathrm{d}F(\omega')\right]$$

Define ω_R such that $V(\underline{\omega}, 0) \equiv V(\omega_R, 1)$, so ω_R is the reservation wage for a job with an NCA. Denote by $\bar{\tau}_1$ and $\bar{\tau}_0$ the expected duration of a job spell with an NCA and without, respectively. Then $\tilde{L_1} \equiv L/\bar{\tau}_1$ is the expected flow cost associated with accepting an NCA.

$$\begin{aligned}
V(\underline{\omega}, 0) &\equiv V(\omega_R, 1) \\
\Rightarrow \quad & \underline{\omega} + \beta\alpha\left[\lambda \int_{\omega_R}^\infty V(\omega', 1) - V(\underline{\omega}, 0)\mathrm{d}F(\omega') + (1 - \lambda)\int_{\underline{\omega}}^\infty V(\omega', 0) - V(\underline{\omega}, 0)\mathrm{d}F(\omega')\right] \\
= \quad & \omega_R + \beta\alpha\left[\left[\lambda \int_{\omega_R + \tilde{L_1}}^\infty V(\omega', 1) - V(\omega_R, 1)\mathrm{d}F(\omega')\right] + (1 - \lambda)\left[\int_{\omega_R + \tilde{L_0}}^\infty V(\omega', 0) - V(\omega_R, 1)\mathrm{d}F(\omega')\right]\right]
\end{aligned}$$

The Compensating Wage Differential for Non-Compete Agreements

$$
\begin{aligned}
\Rightarrow \frac{\underline{\omega} - \omega_R}{\beta\alpha} &= \lambda\left[\int_{\omega_R+\tilde{L}_1}^{\infty} V(\omega', 1) - V(\omega_R, 1)\mathrm{d}F(\omega') - \int_{\omega_R}^{\infty} V(\omega', 1) - V(\underline{\omega}, 0)\mathrm{d}F(\omega')\right] \\
&\quad +(1-\lambda)\left[\int_{\omega_R+\tilde{L}_0}^{\infty} V(\omega', 0) - V(\omega_R, 1)\mathrm{d}F(\omega') - \int_{\underline{\omega}}^{\infty} V(\omega', 0) - V(\underline{\omega}, 0)\mathrm{d}F(\omega')\right]
\end{aligned}
$$

$$
\Rightarrow \omega_R = \underline{\omega} + \beta\alpha\left[\lambda \underbrace{\int_{\omega_R}^{\omega_R+\tilde{L}_1} V(\omega', 1) - V(\underline{\omega}, 0)\mathrm{d}F(\omega')}_{>0} + (1-\lambda) \underbrace{\int_{\underline{\omega}}^{\omega_R+\tilde{L}_0} V(\omega', 0) - V(\underline{\omega}, 0)\mathrm{d}F(\omega')}_{>0}\right]
$$

The compensating differential for accepting an NCA is $\omega_R - \underline{\omega}$, which is strictly positive whenever $L > 0$.

(Partial) Equilibrium Use of Non-Compete Agreements

For much of the following, it is simpler to present the discussion in terms of the static decision problem faced in each period of the dynamic model presented above. Relaxing the assumption that the arrival rate of job offers is independent of non-compete status, suppose there are two different arrival rates for job offers: α_l is the arrival rate for offers from other firms in the worker's local labor market, and α_o is the arrival rate for offers in other labor markets. Workers with non-compete agreements are forbidden from searching in the local labor market, so they receive offers at the rate α_o, while workers without non-compete agreements receive offers at the rate $(\alpha_l + \alpha_o)$. Assume for simplicity that the proportion of job vacancies with non-compete agreements, λ, is the same in each market. We will address this assumption later, but this simple model demonstrates the relevant point at hand. To generalize further, suppose that wages are drawn from different arbitrary distributions, F_1 for jobs with non-compete agreements and F_0 for jobs without.

Workers transition into jobs with non-compete agreements at the rate $(\alpha_l + \alpha_o)\lambda(1 - F_1(\omega_R))$, and out of jobs with non-compete agreements at the rate $\alpha_o(1 - \lambda)(1 - F_0(\omega_R + \tilde{L}_0))$. The derivative of the proportion of workers with non-compete agreements is:

$$\dot{\lambda} = (\alpha_l + \alpha_o)\lambda(1 - F_1(\omega_R)) - \alpha_o(1 - \lambda)(1 - F_0(\omega_R + \tilde{L}_0))$$

which implies that in steady state:

$$\lambda^\star = \frac{\alpha_o(1 - F_0(\omega_R + \tilde{L}_0))}{(\alpha_l + \alpha_o)(1 - F_1(\omega_R)) + \alpha_o(1 - F_0(\omega_R + \tilde{L}_0))}$$

The implication of this is that as long as $F_0(\omega_R + \tilde{L}_0) < 1$, the steady-state will involve a strictly positive fraction of workers with non-compete agreements, even if wages for all jobs are drawn from the same distribution F.

One can also express the distribution of wages that would be observed in a cross-sectional sample of workers, $G(\omega)$. The flow of workers into jobs with non-compete agreements at a wage no greater than ω is $(\alpha_l + \alpha_o)\lambda(F_1(\omega) - F_1(\omega_R))$, while the flow of workers out of jobs with non-compete agreements is $\alpha_o(1-\lambda)G(\omega)(1-F_0(\omega_R+\tilde{L}_0))$. At the steady state:

$$G(\omega) = \frac{(\alpha_l + \alpha_o)\lambda(F_1(\omega) - F_1(\omega_R))}{\alpha_o(1 - \lambda)(1 - F_0(\omega_R + \tilde{L}_0))}$$

And from the steady-state $\dot{\lambda} = 0$ condition:

$$G^\star(\omega) = \frac{F_1(\omega) - F_1(\omega_R)}{1 - F_1(\omega_R)}$$

The Effect of Non-Compete Agreements on Mobility

Using $G^\star$ one can express the rate of job-to-job transition for a worker with a given wage $\underline{\omega}$. The total hazard rate for exiting a job without a non-compete agreement is:

$$\Lambda_0 = (\alpha_l + \alpha_o)\left[(1-\lambda)\int_{\underline{\omega}}^{\infty}[1 - F_0(\omega)]\,\mathrm{d}G^\star(\omega) + (\lambda)\int_{\omega_R}^{\infty}[1 - F_1(\omega)]\,\mathrm{d}G^\star(\omega)\right]$$

and with a non-compete agreement the hazard rate is:

$$\Lambda_1 = (\alpha_o)\left[(1-\lambda)\int_{\omega_R+\tilde{L}_0}^{\infty}[1 - F_0(\omega)]\,\mathrm{d}G^\star(\omega) + (\lambda)\int_{\omega_R+\tilde{L}_1}^{\infty}[1 - F_1(\omega)]\,\mathrm{d}G^\star(\omega)\right]$$

$$\begin{aligned}
\Lambda_0 - \Lambda_1 \quad = \quad & \underbrace{\alpha_l(1-\lambda)\int_{\underline{\omega}}^{\infty}[1 - F_0(\omega)]\,\mathrm{d}G^\star(\omega)}_{>0} + \underbrace{\alpha_o(1-\lambda)\int_{\underline{\omega}}^{\omega_R+\tilde{L}_0}[1 - F_0(\omega)]\,\mathrm{d}G^\star(\omega)}_{>0} \\
& + \underbrace{\alpha_l\lambda\int_{\omega_R}^{\infty}[1 - F_1(\omega)]\,\mathrm{d}G^\star(\omega)}_{>0} + \underbrace{\alpha_o\lambda\int_{\omega_R}^{\omega_R+\tilde{L}_1}[1 - F_1(\omega)]\,\mathrm{d}G^\star(\omega)}_{>0}
\end{aligned}$$

The survivor function for jobs with non-compete agreements, as a function of ω, first order stochastically dominates the survivor function for jobs without non-competes, implying that non-compete clauses lengthen job-spells and reduce the frequency of job transitions. This holds true even if $F_0 = F_1$.

It is also possible to decompose this difference in hazard rate into the component due to the difference in offer arrival rates and the component due to the fixed cost of job transitions due to non-compete agreements:

$$\begin{aligned}
\Lambda_0 - \Lambda_1 \quad = \quad & \overbrace{\alpha_l(1-\lambda)\int_{\underline{\omega}}^{\infty}[1-F_0(\omega)]\,\mathrm{d}G^\star(\omega) + \alpha_o(1-\lambda)\int_{\underline{\omega}}^{\omega_R}[1-F_0(\omega)]\,\mathrm{d}G^\star(\omega) + \alpha_l\lambda\int_{\omega_R}^{\infty}[1-F_1(\omega)]\,\mathrm{d}G^\star(\omega)}^{\text{Component due to difference in offer arrival rates, } > 0} \\
& \underbrace{+\alpha_o\left[(1-\lambda)\int_{\omega_R}^{\omega_R+\tilde{L}_0}[1-F_0(\omega)]\,\mathrm{d}G^\star(\omega) + \lambda\int_{\omega_R}^{\omega_R+\tilde{L}_1}[1-F_1(\omega)]\,\mathrm{d}G^\star(\omega)\right]}_{\text{Component due to } L \text{ transition cost, } > 0}
\end{aligned}$$

There are two channels through which non-compete agreements can reduce mobility. The first component occurs if the offer arrival rate is lower for workers who have signed non-compete agreements, even if job transition costs do not depend on non-compete agreements. The second component shows that increased transition costs associated with non-compete agreements reduce mobility, even if they have no effect on offer arrival rates. Since both components are strictly positive, $\mathbb{E}(\tau_1) > \mathbb{E}(\tau_0)$ and $\mathbb{E}(\tilde{L_1}) < \mathbb{E}(\tilde{L_0})$ whenever $L > 0$. This model assumes that wages remain fixed over time. As we discuss below, if non-compete agreements affect the rate of change of wages over time, the tenure effect could have one or more crossing points.

Labor Demand with Non-Compete Agreements

Since workers require higher wages to accept jobs with non-compete restrictions, in equilibrium such agreements can only exist if firms are willing to pay higher wages. There are several potential explanations that are consistent with firms paying higher wages to impose non-compete agreements upon workers. One is that search is costly, and non-compete agreements increase the expected duration of job-spells, decreasing the average search costs to a firm of keeping a position filled. Another explanation is that increasing the expected duration of the job spell increases the productivity of the worker. This could occur if physicians who expect to remain at a practice for a longer time are more likely to form personal relationships with their patients that increase their productivity, a form of learning by doing. Another way in which firms benefit from non-compete agreements is that, for highly specialized workers like physicians, non-compete agreements give firms local monopsony power. Since most labor contracts are not long-term, monopsonistic power affects negotiations subsequent to the non-compete agreement. It could affect, for example, the dynamic wage profiles of workers who are bound to them. This increase in future bargaining power is valuable to firms, and is another reason why they may be willing to pay higher wages.

There are also potential secondary effects that could cause wages to differ depending on whether a worker has a non-compete agreement. For example, differences in either the rates or returns to learning by doing could affect the elasticity of labor supply. Hansen and Imrohoroglu (2009), for example, show that when the impact of learning by doing is greater, labor supply is more elastic. Since physicians often have some flexibility over their choice of hours, a change in the elasticity of labor supply could affect both the number of hours worked and wage. Thus non-compete agreements have the potential to affect both the intensive and extensive margins of labor supply.

Given these incentives, an important question is: why do we observe the use of non-compete agreements for physicians, but less frequently in many other professions? One difference between primary care physicians and many other professions is that physicians generate substantial intangible value over time as patients become loyal them. In many cases this loyalty is to an individual physician, rather than to a practice. Without a non-compete clause, a physician has the power to substantially affect the value of the firm by taking patients away from it. Non-compete clauses are one way in which firms can insure against such changes to the intangible assets of the firm.

To further investigate firms' incentives and bargaining, consider the following model of firm behavior: There are two types of jobs, those with non-competes and those without. The cost to the firm of operating a job with a non-compete clause, k_1, is at least as high as that of operating a job without, k_0. For simplicity, assume that firms pay this cost of operating a job regardless of whether the position is vacant or filled. Output produced in a job with a non-compete is a and without a non-compete output is ay, where $0 < y \leq 1$ Net output satisfies $a - k_1 \geq ay - k_0$, which implies that $y \leq 1 - (k_1 - k_0)$. For a firm that uses non-compete agreements, the expected profit from recruiting a worker is:

$$J_1(\omega_1) = \frac{a - k_1 - \omega_1 - \alpha_o \left[\lambda(1 - F_1(\omega_1 + \tilde{L_1})) + (1 - \lambda)(1 - F_0(\omega_1 + \tilde{L_0}))\right]}{1 - \beta}$$

$$J_0(\omega_0) = \frac{ay - k_0 - \omega_0 - (\alpha_l + \alpha_o)\left[\lambda(1 - F_1(\omega_0 + \tilde{L_1})) + (1 - \lambda)(1 - F_0(\omega_0)\right]}{1 - \beta}$$

Firms offer contracts with non-compete agreements if $J_1 \geq J_0$, or

$$\begin{aligned} \overbrace{a(1-y)}^{\text{Productivity Gain}} \quad & + \quad \overbrace{\alpha_l \lambda \left[F_0(\omega_0) - F_1(\omega_1 + \tilde{L_1})\right] + \alpha_0(1-\lambda)\left[F_0(\omega_0) - F_0(\omega_0 + \tilde{L_0})\right] + \alpha_l \left[1 - F_0(\omega_0)\right]}^{\text{Increase in Expected Profit from Lower Probability of Job Switch}} \\ & > \quad \underbrace{(\omega_1 - \omega_0) + (k_1 - k_0)}_{\text{Wage and Cost Increases}} \end{aligned}$$

Bargaining and Wage Determination

The determination of wages follows from Pissarides (1994), where the surplus from a worker and firm match is split according to the relative bargaining power of each. This approach is more appealing than the approaches used in other partial models of on-the-job search, [11] because these models ignore the effect of search on local monopsony power, which invalidates the assumption that wages equal the value of marginal product. The effect of non-compete agreements on local monopsony power is precisely what we are interested in investigating.

For a worker who is not constrained by a non-compete agreement, whose expected return to current employment is V_0, the equilibrium wage for a job without a non-compete agreement is given by:

$$\omega_{00} = V_0 + \theta\left[ay - k_0 - V_0\right] - (1-\theta)(\alpha_o + \alpha_l)\left[\lambda(V_1^{'} - V_0) + (1-\lambda)(V_0^{'} - V_0)\right]$$

where θ is the fraction of the net surplus created by the job that goes to the worker.

[11] Including Burdett (1978), Mortensen (1986), Jovanovic (1984), and Burgess (1991)

Similarly, for a worker who switches into a job with a non-compete agreement:

$$\omega_{01} = V_0 + \theta\left[a - k_1 - V_0\right] - (1-\theta)\alpha_o\left[\lambda(V_1^{'} - V_1 - L) + (1-\lambda)(V_0^{'} - V_1 - L)\right]$$

In this model, for workers with non-compete agreements there are two distinct effects that tend to increase wages. The first is the increase in productivity, $a(1-y)$, due to increased learning on the job. The second is due to the reduction in search intensity by the factor α_l, which reduces the absolute value of the negative term.

Dynamic Effects

To be explicit about the dynamic effects, denote by $a(\tau)$ and $\omega(\tau)$ productivity and wages as functions of job tenure. Assume that $a(\tau)$ is continuous, weakly increasing, and differentiable, with a finite upper bound. Subsequent to the initial wage bargaining problem, firms that have negotiated non-compete agreements with workers have greater local monopsony power, which reduces the magnitude of θ for all bargaining that takes place after the initial agreement. Denote by $\bar{\theta}$ and $\underline{\theta}$ the initial and subsequent bargaining power of workers who accept non-compete agreements. Suppose that wages do not fall, but that bargaining takes place over the magnitude of wage increases. Intuitively, $\omega_{01}(\tau)$ is determined by two components. The first is proportional to the change in productivity, $\omega_{01}(0) + \underline{\theta}(a(\tau) - a(0))$ and is clearly positive. The second component is slightly more complicated. The reduction in bargaining power for the worker tends to reduce wages by increasing the firm's share of the expected gains from on-the-job search. Simultaneously, as wages rise with tenure at the firm, the probability that a wage offer will dominate the current wage falls, reducing the expected gains to search. The change in bargaining power affects the level of wages, while the expected gains from search depends on the rate of change of wages.

The more interesting question is: are wages at tenure τ affected by non-compete agreements. Again, there are two distinct components: one due to bargaining over productivity gains, and another due to bargaining over changes in the expected gains from on-the-job search. The effect of productivity gains on wages is

$$\omega_{01}(0) - \omega_{00}(0) + a(\tau)\left[\underline{\theta} - \bar{\theta}y\right]$$

From the static analysis, we expect that $\omega_{01}(0) - \omega_{00}(0) > 0$. The second term has ambiguous direction, and there are two possible scenarios. If the reduction in bargaining power is very large then $\underline{\theta}$ will be small relative to $\bar{\theta}y$, and the wage profile will tend to flatter for workers with non-compete agreements. That is, the initial compensating differential for accepting a non-compete agreement will gradually dissipate, and the difference in wages could become negative as τ increases. Since firms are still constrained by workers' opportunities outside the local market, it would require fairly extreme and implausible assumptions for $\underline{\theta}$ to fall to zero if $\bar{\theta} > 0$. Conversely, if the effect of non-compete agreements on productivity through learning by doing is large relative to the change in bargaining power, then y is relatively small and $\underline{\theta} - \bar{\theta}y > 0$. In this scenario the dynamic wage profiles for workers with non-compete agreements could both start above those of workers without non-competes, and rise at a faster rate. The second component, due to changes in the expected gains from on-the-job search, tends to increase wages as τ increases and the expected gains to switching decrease. The difference in the rate at which wages increase depends on the sign of the first component, and so this second component tends in the same direction as the first component in each of the cases. Thus the total net effect on the dynamic wage path depends on the effect that non-compete agreements have on learning-by-doing relative to their effects on bargaining through an increase in monopsony power by firms.

Although the magnitude of each effect is not separately identifiable with our data, this model suggests a testable empirical hypothesis with direct policy relevance. If the wage profile of workers with non-compete agreements is steeper than those without, the data suggest that the productivity gains through increased learning-by-doing, or some similar channel, outweigh the effects of local monopsony power by firms. A related testable hypothesis is that if local monopsony power by firms has a relatively small effect on the dynamic wage profile, then the rate of job-to-job switching should fall faster with tenure for workers with non-compete clauses than for those without. The static analysis suggested that the level of job-to-job switching should be lower for workers with non-compete agreements as well. Of course, it is intuitively likely that there is a crossing point $\tau^{\star}$ after which job-to-job switches no longer have any gain, so the hypothesis is relevant to the lower end of the support of the distribution of job tenure.

2.4 Data and Estimation Approach

2.4.1 Data

This research draws on data from the Physician Perspectives on Managed Care Survey, conducted in 2007 and funded by the Agency for Healthcare Research and

Quality (AHRQ), the California Endowment, and the Commonwealth Fund. The study sampled physicians who were listed in the American Medical Association (AMA) Masterfile as providing patient care in the specialties of general family practice, general practice, general internal medicine, and general pediatrics in five states, California, Texas, Illinois, Georgia, and Pennsylvania, which were selected to be representative of a variety of practice environments. Using a state-based sample rather than a national survey permitted collection of larger samples for local market areas. Excluded from the target population were residents, fellows, physicians not in clinical practice, and those over 70 years of age. Pediatricians and minority physicians were over-sampled.[12]

The AMA database provides information on physician location and contact information, specialty and training, age, and, where information was available, race. Telephone calls verified contact information and whether sample physicians were providing patient care. A multi-mode (mail and web) self-administered survey was conducted. A packet was sent by Federal Express to a total of 2,831 physicians containing a mail survey accompanied by an advance letter, a pre-paid business return envelope and an honorarium check of $100. Physicians were given the option of responding by web. Follow-up was conducted for those physicians who did not respond, with separate follow-up with those who did not respond but cashed their checks. Altogether, a total of 1,967 usable responses were received, 216 (11%) of which were by web. The overall response rate was 69.8%. Responses varied by sampled specialty from 76% for pediatricians to 64% for specialists in internal medicine. Base sampling weights were assigned to each physician based on the inverse of

[12]While pediatricians made up approximately 23in the study states, they were oversampled so that they made up 45oversampled to ensure a sample of at least 200 responses in order to provide a meaningful basis for analyses examining this subgroup.

their probability of selection and then adjusted for the probability of non-response and the probability of being sampled based on race.

The survey questionnaire included questions on the following topics: physician characteristics, practice characteristics, physician demographics, practice revenues, physician income, administrative controls, practice use of electronic medical records and HIT, and patient vignettes.

Physicians were asked how many medical practices they worked in and their ownership status in their main practice. If they responded they were a sole-owner, the survey proceeded to questions about general practice characteristics. However, if the physician indicated that they were not a sole-owner, they were asked about their employment status and the following question regarding non-competes:
Were you to leave your (main) practice, would you be subject to a non-compete clause?
☐ Yes
☐ No
Physicians were also asked the year in which they began work in their main practice, from which we have information about practice tenure.

2.4.2 Estimation Approach

In order to estimate the compensating wage differential for accepting a non-compete agreement, it would be ideal to have longitudinal data with job-switches, which would allow one to control for unobserved heterogeneity across physicians. An important source of unobserved heterogeneity is the physician's latent preference for workplace mobility, which is likely to characterize sorting behavior between firms that impose non-compete agreements and workers. Failure to control for the unobserved factors that affect this assortative matching implies that our estimates are measures of the average slope of the equilibrium envelope function with respect to wages and the strength of non-compete restrictions. As are almost all estimates of compensating differentials, this is an estimate of the average effect of treatment on the treated. Although the data cannot identify workers' preferences for non-compete agreements, they are nonetheless informative about the market equilibrium. We are particularly interested in the market equilibrium here, rather than in preferences, because of the concern about potential anticompetitive effects of non-compete agreements. Thus the data limitations are less problematic here than in many other cross-sectional estimates of compensating wage differentials.

Our estimates of the wage effects are limited to the selected sample of workers who chose to accept non-compete agreements. The equilibrium wage price of a non-compete restriction is based on the preferences of the marginal worker in a market, and so the wage effects would likely change if a policy change altered the expected benefits to firms of imposing non-competes. Our estimates of the wage effects can be interpreted as a lower bound on workers' preferences for accepting non-compete restrictions, as those workers with stronger preferences against non-competes are less likely to have accepted them.

In examining adoption of non-compete agreements, we focus on individual physicians as we lack complete practice-level data. We first estimate a model of the use of non-compete agreements, in which the dependent variable is whether or not the worker is bound by a non-compete agreement, and independent variables include observable physician characteristics such as age, specialty, whether the physician plans to retire within 3 years, a control for US or foreign medical training, race, and ownership status. To a limited extent we control for preferences for mobility by including a variable that equals one if the physician has an employed spouse or partner, which may affect the cost of geographic mobility.[13] We also control for practice and local market conditions, including whether the practice is office-based, free-standing, or associated with a University; whether it is a small practice with 3 or fewer physicians; whether there are physicians with different specialties at the practice; the percent of the practice's patients that are insured; the log of physicians per capita in the local market; and the log of household income in the market. We control for the state in which the practice is located, to account for differences across states in the strength of non-compete enforcement, and an interaction between state and ownership status. The interaction terms capture possible state-level differences in the treatment of part-owners versus employees. Our state dummies may capture not only differences in the ability to enforce non-competes, but also other unobserved state characteristics not already controlled-for by market characteristics. An alternative specification uses the measure of non-compete restrictiveness from Bishara (2011). Using state dummies alone avoids possible biases that could be associated with incorrectly assessing differences in legal treatment, but cannot identify the answer to a key question of interest: what effects would a change in the restrictiveness of non-compete agreements have on the labor market?

We expect that the benefit to a practice of imposing a non-compete agreement

[13]We also tried controlling for geographic differences in mobility preferences using census data to estimate the probability that a person in a given zip code will move to another zip code more than 100 miles away in a given year. This variable had little explanatory power and was dropped from the analysis.

depends upon whether the physician is a part-owner or simply an employee of the practice. If a part-owner were to leave a practice and compete against it, they may also reduce the value of their equity share in the practice. This incentive to maintain the value of their stake in the practice in the absence of a non-compete agreement increases the marginal cost to the physician of competing against the practice, and thus decreases the marginal benefit to the practice of imposing a non-compete. As a consequence, we hypothesize a practice will be more likely to require a non-compete for employee physicians.

We identify the effect of non-compete agreements on equilibrium wages using cross-sectional variation in hourly earnings and observable characteristics, including similar physician, practice, and market conditions. Hourly earnings are calculated as total annual earnings divided by the average number of hours spent working each week times 50. Hours worked includes time spent treating patients as well as other administrative work. To capture differences in costs of living, we use the Medicare geographic practice expense index.[14] We estimate the model separately for the employees and part-owners, as well as for the combined sample.

We also estimate how the equilibrium compensating differential for accepting a non-compete agreement changes with tenure at the practice. This model is used to test the hypothesis from the theoretical model that non-compete agreements affect the dynamic wage profiles of workers, and to provide evidence on whether the predominant channel through which they affect wage profiles is through changes in monopsonistic bargaining power or by affecting productivity, possibly through

[14] An alternative would be use CPI data. However, these data are only available for a limited number of MSAs and on a regional basis, whereas Medicare geographic practice expense index is available at the local level nationwide. Comparing this index with the CPI, the two indexes generally track closely.

learning by doing.

To investigate the relationship between NCAs and physician mobility, we use responses to the question: What year did you begin working in this practice? This variable is of course a right-censored measure of tenure for every respondent in the sample, and we never observe the true length of a job-spell in the survey. Still, we can use the responses to this question, along with potential experience, to infer whether a physician has switched practices at least one time in his or her career. We then estimate patterns between the use of NCAs and this mobility variable for physicians with various levels of potential experience.

To account for differences in the enforceability of NCAs in different states, we use a continuous measure for NCAs rather than a dummy variable. The variable is constructed for each state by dividing the score from Bishara (2011) by the highest score for any state, so that the variable ranges between zero and one. A one unit increase in this variable has the convenient interpretation as the difference between the most restrictive and least restrictive laws in place.

2.5 Results

Table 2.3 reports the share of physicians with non-compete agreements in each state in the sample. The use of non-compete agreements varies substantially across states, ranging from 31.3% in California to 60.6% in Pennsylvania, with an average of 45.1% of all physicians in the sample subject to non-compete agreements. This variation in usage is consistent with differences in enforceability causing differences across states in the benefits of imposing non-compete agreements. We present further evidence of this in Table 2.9. Across the five states in our sample, the correlation between the fraction of physicians with non-competes and the overall strength of state enforceability is highly positive, 0.93. Non-competes are also used more frequently for physicians who are employees (49.2%) rather than part-owners (43.1%). This is consistent with the hypothesis that part-owners have some deterrent to competing against their current practice because doing so could devalue their share of the practice's equity. Thus the net benefit to a firm of imposing a non-compete on a part-owner may be lower.

Table 2.3: % of Respondents with Non-Compete Clauses, By State and Employment Status

	Full Sample		Employees		Part Owners	
California	511	31.3%	225	29.8%	206	36.9%
Georgia	120	51.7%	51	60.8%	53	43.4%
Illinois	217	52.1%	124	50.0%	73	54.8%
Pennsylvania	231	60.6%	147	66.0%	62	54.8%
Texas	268	49.6%	129	58.9%	97	45.4%
All States	1347	45.1%	723	49.2%	534	43.1%

The use of non-compete agreements has also been increasing rapidly over time. The last column of Table 2.4 shows that for physicians who graduated medical school within 5 years of the survey, about 61% are subject to non-compete agreements. Of the physicians who graduated during the 1990's, approximately 48% are subject to non-competes, and only 43% of physicians who graduated before 1990.

Table 2.4: % of Respondents with Non-Compete Clauses, By Potential Experience and Practice Tenure

	Tenure					
Potential Exp.	5 of Fewer	6 to 10	11 to 15	16 to 20	More than 20	
Less than 5	61.0%					61.0%
6 to 10	44.0%	50.0%				47.2%
11 to 15	47.6%	42.7%	56.8%			48.2%
16 to 20	39.3%	40.4%	41.7%	51.6%		43.4%
More than 20	38.8%	46.7%	41.8%	37.5%	43.7%	42.3%
	44.6%	46.1%	47.0%	43.2%	43.7%	45.1%

This change over time is largely due to shifts in the characteristics of practices. Table 2.5 shows the effects that physician, practice, and market characteristics have on the probability that a physician is subject to a non-compete agreement using a logit regression where the dependent variable is 1 if the physician is subject to a non-compete. Physicians in office-based practices are about 13 percentage points more likely to have a non-compete relative to hospital-based and community care physicians. Physicians in free-standing practices or university practices are significantly less likely to have non-compete agreements, about 27 and 23 percentage points, respectively. Small practices, with fewer than four physicians are significantly less likely to impose non-compete agreements as well, by about 12 percentage points. The effect of ownership status on the use of non-compete was clearly negative in the summary statistics in Table 2.3, and marginal effect from the logit regression is an 11 percentage point reduction for part-owners. The most substantial effects are for physicians who indicate that they are nearing retirement, who are about 28 percentage points less likely to have a non-compete. An indication of preferences for mobility, physicians with employed spouses were about 9 percentage points less likely to have accepted a job with a non-compete agreement. Physicians in Pennsylvania were significantly more likely to be subject to non-compete agreements than those in Georgia. Model 3 shows that the strength of enforceability of non-competes in the state has a large and significant effect on the usage of non-competes. The variable 'State Restrictiveness Score (Bishara)' scales the state ratings from Bishara (2011) so that the values range between 0 and 1, with the most restrictive state having a value of 1. We find that physicians in the state with the least enforceable non-compete laws are about 24 percentage points less likely to have a non-compete than those in the state with the most restrictive laws.

Perhaps the most important result in Table 2.5 from a policy perspective is the significant effect that the relative supply of physicians per capita has on the usage of non-compete agreements. A one log point increase the number of physicians per 100,000 residents reduces the probability that a physician will have a non-compete agreement by about 25 percentage points. Since physician supply per capita is highly correlated with total population, this result is consistent with firms in urban areas being less concerned about the effects of a physician exiting the practice and competing against it. The result suggests that physicians in rural areas are more likely to be constrained by non-compete agreements. If selection on the basis of preferences for occupational mobility affects rural and urban areas differently, state enforcement of non-compete agreements may have implications for the disparities that exist in the relative supply of physicians in urban and rural areas.

Estimates of the static compensating wage differential for accepting a non-compete agreement are presented in Table 2.6. In Model 4 we find that non-compete agreements increase hourly earnings by about 11% on average. In Model 5 we con-

Table 2.5: Logit Models, Non-Compete Clause
Dependent Variable: Non-Compete Agreement

	(1)			(2)			(3)		
	Marg. Effect	S.E.		Marg. Effect	S.E.		Marg. Effect	S.E.	
State Restrictiveness Score (Bishara)							0.24	0.05	***
Specialist: Internal Medicine	0.04	0.04		0.04	0.05		0.05	0.05	
Specialist: Pediatrics	0.06	0.04		0.05	0.04		0.05	0.04	
Specialist: Secondary Specialty	0.02	0.04		0.02	0.04		0.04	0.04	
Plan to Retire within 3 Years	-0.28	0.09	***	-0.27	0.10	***	-0.28	0.09	***
Office-Based Practice	0.13	0.04	***	0.14	0.04	***	0.16	0.04	***
Free-Standing Practice	-0.27	0.11	**	-0.26	0.12	**	-0.26	0.12	**
University Practice	-0.23	0.06	***	-0.23	0.06	***	-0.21	0.06	***
Small Practice (1-3)	-0.12	0.04	***	-0.11	0.04	***	-0.13	0.04	***
Multi-Specialty Practice	0.05	0.03		0.04	0.03		0.06	0.03	*
Part Owner	-0.11	0.03	***	-0.18	0.07	**	-0.12	0.04	***
Independent Contractor at Practice	-0.20	0.06	***	-0.20	0.06	***	-0.21	0.06	***
% Patients Uninsured	0.00	0.00		0.00	0.00		0.00	0.00	
Employed Spouse/Partner	-0.09	0.03	**	-0.08	0.03	**	-0.08	0.03	**
US Med School	0.06	0.05		0.06	0.05		0.06	0.05	
Potential Experience	-0.01	0.00	*	-0.01	0.00	*	-0.01	0.00	*
Percent Urban	0.00	0.00		0.00	0.00		0.00	0.00	
Log Physicians per Capita	-0.25	0.07	***	-0.23	0.07	***	-0.21	0.07	***
Log Median Household Income	0.09	0.04	**	0.09	0.04	**	0.07	0.04	
State: PA	0.22	0.06	***	0.21	0.07	***			
State: CA	-0.07	0.05		-0.16	0.06	**			
State: TX	0.06	0.05		0.07	0.07				
State: IL	0.11	0.06	*	0.04	0.08				
State: PA×Part-Owner				-0.01	0.12				
State: CA×Part-Owner				0.20	0.09	**			
State: TX×Part-Owner				-0.05	0.10				
State: IL×Part-Owner				0.15	0.11				
N	1175			1175			1093		
Adj. R^2	0.106			0.113			0.108		

Note: Marginal effects reported. All standard errors are White-Huber heteroskedasticity-adjusted. * significant at 10%, ** significant at 5%, *** significant at 1%. All models also include age and race controls.

trol for the restrictiveness of state laws, rather than using a dummy variable for NCAs, and find that wages for workers with non-competes in the most restrictive state are about 17% higher than wages for workers without non-compete agreements. Both of these estimates are significant at the 1% level. Models 5 and 6 separately estimate the effect for employees and part-owners. We find that the compensating differential for employees is about a 20% increase in hourly earnings, and there is no statistically significant compensating differential for part-owners, although the coefficient is positive.

Coefficients on other covariates are largely consistent with theory. The preferred model is (5), in which we find that hourly earnings are about 8 percent higher for pediatricians, about 22 percent higher for very senior physicians who are nearing retirement, and about 14 percent higher for physicians associated with universities. Wages are also about 9 percent higher for part-owners, and 19 percent lower for independent contractors. Our findings are consistent with data from the Bureau of Labor Statistics, which suggest that self-employed practice owners earn higher median incomes than employed physicians. We also find that the elasticity of wages with respect to the supply of physicians per capita in the state in -0.19. We also control for geographic variation in prices with an index of physician practice costs.

Table 2.7 presents estimates of the dynamic effects that NCAs have on the career wage paths of physicians. The theoretical model suggested a testable hypothesis about the relative size of productivity gains and monopsonistic effects based on the difference between the slope of the wage profiles of workers with and without NCAs. To the extent that the net effect is significantly different from zero, this would suggest that one of these two predicted effects has substantially more influence on

Table 2.6: Static Hourly Earnings Models
Dependent Variable: Log Hourly Earnings

	(4) Full Sample			(5) Full Sample			(6) Employees			(7) Part-Owners		
	Coeff.	S.E.		Coeff.	S.E.		Coeff.	S.E.		Coeff.	S.E.	
Non-Compete Clause	0.11	0.04	***									
State Restrictiveness Score×Non-Compete				0.17	0.06	***	0.20	0.09	**	0.12	0.10	
Specialist: Internal Medicine	0.01	0.05		0.01	0.05		0.09	0.07		-0.12	0.08	
Specialist: Pediatrics	0.08	0.04	*	0.08	0.04	*	0.08	0.06		0.05	0.07	
Specialist: Secondary Specialty	0.06	0.05		0.05	0.05		0.07	0.06		0.03	0.08	
Plan to Retire within 3 Years	0.23	0.13	*	0.22	0.12	*	0.20	0.17		0.36	0.29	
Office-Based Practice	-0.05	0.05		-0.05	0.05		-0.06	0.06		-0.01	0.12	
Free-Standing Practice	0.08	0.17		0.07	0.17		0.04	0.23		0.12	0.30	
University Practice	0.12	0.08		0.14	0.08	*	0.15	0.09	*	-0.12	0.49	
Years of Tenure at Current Practice	0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.01	
Small Practice (1-3)	-0.03	0.04		-0.04	0.04		0.00	0.06		-0.05	0.07	
Potential Experience	0.01	0.01		0.01	0.01		0.00	0.02		0.00	0.02	
Potential Experience Squared	0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00	
Multi-Specialty Practice	-0.07	0.04	*	-0.06	0.04		-0.09	0.06		-0.05	0.06	
Part Owner of Practice	0.09	0.04	**	0.09	0.04	**						
Independent Contractor at Practice	-0.19	0.08	**	-0.19	0.08	**						
Geo. Phys. Cost Index - Practice Exp.	-0.03	0.01	**	-0.03	0.01	**	-0.01	0.02		-0.04	0.02	**
Geo. Phys. Cost Index - Practice Exp. Sq.	0.00	0.00	**	0.00	0.00	**	0.00	0.00		0.00	0.00	**
Log Physicians per Capita	-0.19	0.08	**	-0.18	0.08	**	-0.12	0.12		-0.19	0.12	
Log Median Household Income	-0.02	0.05		-0.02	0.05		-0.09	0.07		0.03	0.08	
State: PA	-0.04	0.07		-0.07	0.08		-0.09	0.11		-0.04	0.11	
State: CA	0.10	0.07		0.14	0.07	*	0.18	0.11		0.05	0.11	
State: TX	0.11	0.07		0.09	0.07		0.10	0.11		0.09	0.10	
State: IL	0.03	0.07		0.00	0.08		-0.08	0.11		0.04	0.11	
Constant	6.60	0.86	***	6.71	0.86	***	6.00	1.20	***	7.06	1.44	***
N	692			690			350			303		
Adj. R^2	0.128			0.128			0.204			0.111		

Note: All standard errors are White-Huber heteroskedasticity-adjusted. * significant at 10%, ** significant at 5%, *** significant at 1%. All models also include controls for age, race, percent of patients who are uninsured, employed spouse, percent urban, and US medical school graduate.

the dynamic wage path than the other. We find that NCAs have a nonlinear effect on the wage path, which causes it to be steeper initially and then fall significantly below the wage path for workers without NCAs. The predicted wage path as a function of potential experience for physicians without NCAs is shown in Figure 2.1 and for physicians with NCAs in Figure 2.2. A comparison of the wage paths in Figure 2.3 shows that wages for physicians with NCAs are not statistically significantly different at very low levels of potential experience, but the rate of increase is significantly higher for physicians with NCAs until they have around 15 years of experience. By that time the hourly earnings of physicians with NCAs are about 11% higher than those without. However, after about 20 years of potential experience the hourly earnings of physicians with NCAs actually fall, erasing the wage gap. This suggests that both hypothesized effects may play some role. The steeper increase in wages with initial experience is consistent with the hypothesis that NCAs increase the rate of learning by doing. However, by mid-career, the effects of monopsony power appear to outweigh the incremental productivity effects, and the wage premium is erased.

Table 2.7: Dynamic Hourly Earnings Models

Dependent Variable: Log Hourly Earnings	(8)		
	Coeff.	S.E.	
Non-Compete Clause	-0.11	0.11	
Non-Compete Clause×Potential Experience	0.0303	0.0135	**
Non-Compete Clause×Potential Experience Sq.	-0.0009	0.0004	**
Specialist: Internal Medicine	0.01	0.06	
Specialist: Pediatrics	0.09	0.04	**
Specialist: Secondary Specialty	0.06	0.05	
Plan to Retire within 3 Years	0.19	0.15	
Office-based practice	-0.04	0.05	
Free Standing practice	0.09	0.13	
University practice	0.12	0.11	
Years of Tenure at Current Practice	0.01	0.00	**
Small Practice (1-3)	-0.01	0.04	
Potential Experience	0.00	0.00	
Multi-Specialty Practice	-0.07	0.04	*
Part Owner of Practice	0.28	0.08	***
Independent Contractor at Practice	0.16	0.08	**
Employed Spouse/Partner	0.00	0.04	
US Med School	-0.01	0.06	
Percent Urban	0.00	0.00	
White, Non-Hispanic	0.00	0.06	
Asian	0.03	0.06	
Black	-0.02	0.11	
Geo. Phys. Cost Index - Practice Exp.	-0.03	0.01	**
Geo. Phys. Cost Index - Practice Exp. Squared	0.00	0.00	**
ln(Primary Care Physicians per 100,000 Residents)	-0.24	0.08	***
ln(Median Household Income)	-0.06	0.05	
State: PA	-0.03	0.08	
State: CA	0.10	0.08	
State: TX	0.13	0.08	*
State: IL	0.07	0.08	
Constant	7.06	0.84	***
N	650		
Adj. R^2	0.130		

Note: All standard errors are White-Huber heteroskedasticity-adjusted. Sample includes physicians who reported between 200 and 4000 annual hours worked and are less than 65 years old. * significant at 10%, ** significant at 5%, *** significant at 1%.

From a policy perspective, one concern is that the cost of compensating differ-

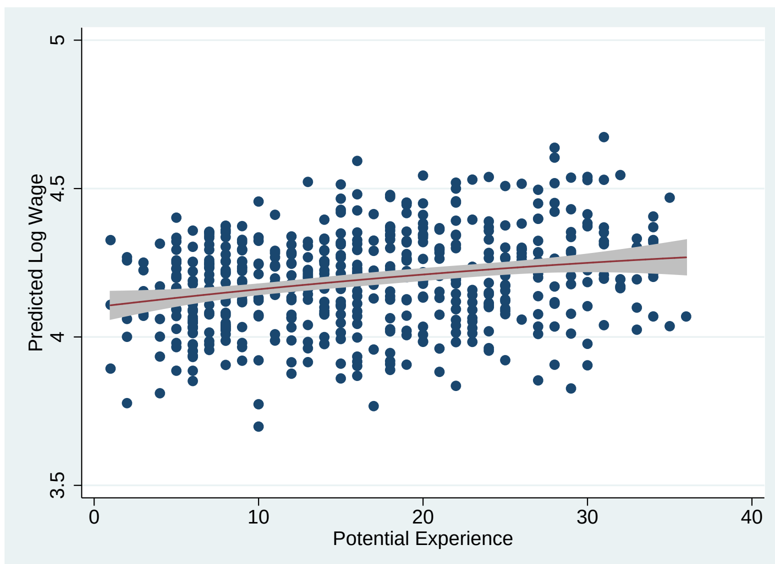
Table 2.8: Pricing Models

	(9)			(10)			(11)		
Dependent Variable:	Log Index Adjusted Price			Log Index Adjusted Price			Log Price		
State Restrictiveness Score (Bishara)	0.36	0.09	***				0.25	0.09	***
Geo. Phys. Cost Index - Practice Exp.							0.30	0.23	
Log Physicians per 100,000 Residents	-0.03	0.10		0.04	0.12		0.06	0.11	
Log Median Household Income	0.14	0.08	*	0.16	0.08	*	0.20	0.09	**
Specialist: Internal Medicine	0.17	0.09	*	0.16	0.09	*	0.18	0.09	**
Specialist: Pediatrics	0.00	0.06		-0.01	0.06		0.01	0.06	
Specialist: Secondary Specialty	0.17	0.08	**	0.17	0.08	**	0.17	0.08	**
Potential Experience	-0.01	0.01		-0.01	0.01		-0.01	0.01	
Potential Experience Sq.	0.00	0.00		0.00	0.00		0.00	0.00	
Office-based practice	0.14	0.13		0.14	0.13		0.12	0.14	
Free-Standing practice	0.11	0.19		0.12	0.18		0.06	0.19	
University practice	0.83	0.25	***	0.85	0.26	***	0.81	0.26	***
Small Practice (1-3)	-0.01	0.06		0.00	0.06		0.02	0.06	
Multi-Specialty Practice	-0.00	0.06		-0.01	0.06		-0.01	0.06	
Employee	0.07	0.06		0.07	0.06		0.06	0.06	
Independent Contractor	0.08	0.11		0.09	0.11		0.09	0.11	
% Managed Care	-0.01	0.00	*	-0.01	0.00	*	-0.01	0.00	*
Percent Urban	0.00	0.00		0.00	0.00		0.00	0.00	
State: PA				0.03	0.11				
State: CA				-0.23	0.10	**			
State: TX				0.08	0.09				
State: IL				0.02	0.10				
Const.	2.67	0.96	***	2.43	1.04	**	1.32	0.99	
N	350			350			350		
Adj R^2	0.15			0.15			0.14		

Note: Prices based on fee for inital office visit with a private commercially-insured patient. Dependent variable in Models 9 and 10 is price divided by geographic physician practice cost index. Dependent variable in Model 11 is price. All standard errors are White-Huber heteroskedasticity-adjusted. * significant at 10%, ** significant at 5%, *** significant at 1%.

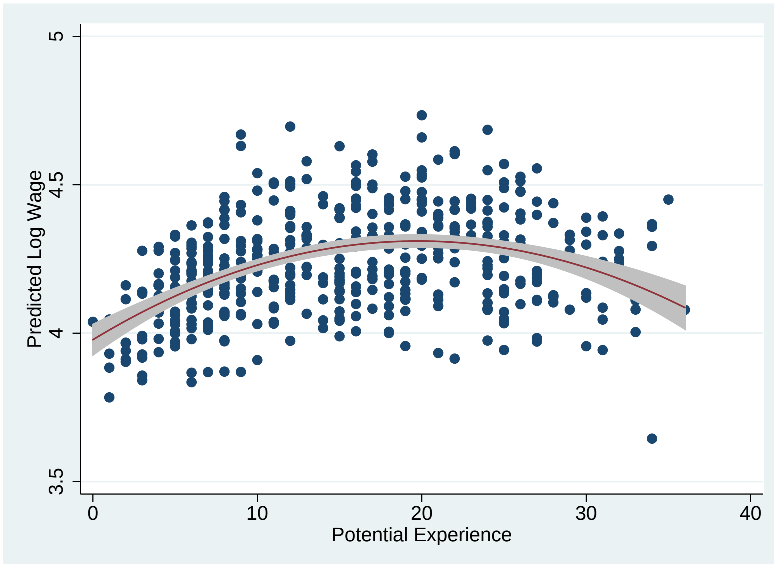
entials are being passed onto consumers in the form of higher prices, as economic theory suggests that it may if firms have bargaining power in the output market. We test for this explicitly using data on the price of an initial patient visit. The survey included the question: "on average, what is your net fee after discount for an initial office visit with a private commercially-insured patient?" Table 2.8 presents estimates of the effect of state enforceability of non-competes on the price of initial office visits. In Models 9 and 10 the dependent variable is the log of the initial visit price divided by the geographic physician practice cost index. In Model 11 the dependent variable is the log of the initial visit price, and the practice cost index is included as an explanatory variable instead. We also control for all other observable characteristics in our data that one would expect to affect prices, including the supply of physicians per capita in the zip code, median household income in the practice zip code, physician specialty, practice setting practice size, and managed care concentration. We find that increasing the state restrictiveness score of non-compete enforcement by one unit, which is equivalent to the difference in enforceability between the least enforceable state in the country (North Dakota) to the most enforceable state in the country (Florida) increases the cost-adjusted price of initial patient visit by about 36%. In Model 11, without cost-adjustment in the dependent variable, the effect is a 25% increase in price. One concern with these models is that all of the variation is at the state level, so if there are characteristics of the state that are not controlled-for in the model, the estimates could be biased. In Model 10 we use state effects instead of controlling for the restrictiveness of non-compete agreements. A comparison of the state effects to the state restrictiveness scores suggests that the cross-state variation is indeed consistent with the enforceability of NCAs. In California, the least enforceable state, prices are about 24% lower, controlling for factors that affect supply and demand. The three states in our sample with the most enforceable non-compete laws are PA, TX, IL. We strongly reject the hypothesis that prices in California as high as they are in any of these states. The second least restrictive state, Georgia, is omitted, and the positive coefficients on the other included states is consistent with our hypothesis, although the small sample size of 350 is insufficient for cross-state comparison between the states with similar restrictiveness scores. This suggests that any omitted state factors either have little effect on prices charged by physicians or are strongly correlated with state laws regarding NCAs.

Figure 2.1: Predicted Wage vs. Potential Experience, Without NCA



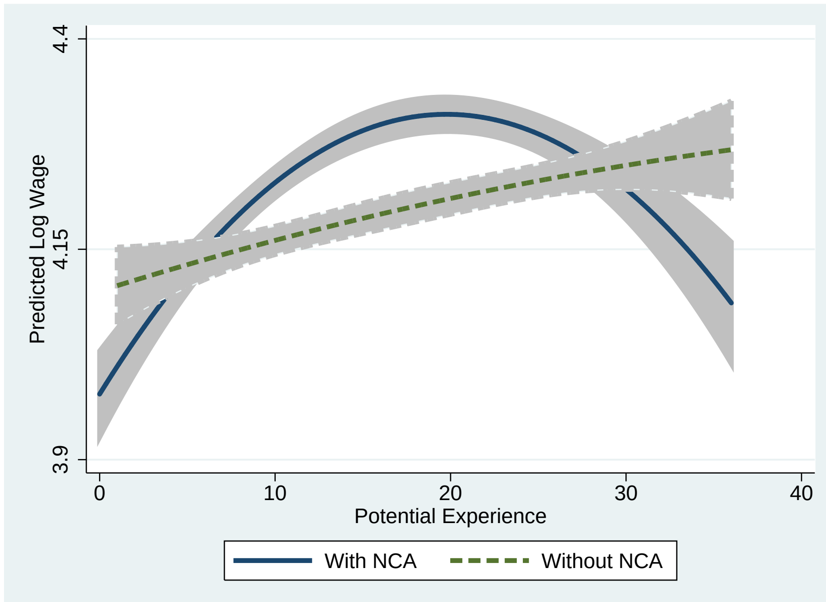
Note: Predicted Wage from Table 2.7. Line is best-fitting quadratic function, with 95% confidence interval.

Figure 2.2: Predicted Wage vs. Potential Experience, With NCA



Note: Predicted Wage from Table 2.7. Line is best-fitting quadratic function, with 95% confidence interval.

Figure 2.3: Comparison of Predicted Wage Paths, With and Without NCAs



Note: Predicted Wage from Table 2.7. Lines are best-fitting quadratic functions, with 95% confidence intervals.

The models in Table 2.9 suggest that physicians assortatively match with practices based on preferences for occupational mobility. We find that physicians who are not subject to NCAs are about 15 percentage points more likely to have switched practices within the last ten years for every 1 unit increase in the Bishara scale. Since the percentage of physicians subject to NCAs is higher in restrictive states, the physicians who are not constrained by NCAs are those with the strongest preferences for mobility.

Table 2.9: Logit Models of Sorting into NCAs

Dependent Variable	(12) Without NCA Moved 10 Years Coeff.	S.E.		(13) With NCA Moved 10 Years Coeff.	S.E.	
Bishara Score	0.15	0.08	*	-0.09	0.09	
Specialist: Internal Medicine	0.13	0.07	*	0.05	0.07	
Specialist: Pediatrics	0.07	0.06		0.04	0.06	
Specialist: Secondary Specialty	0.02	0.07		0.09	0.08	
Office-based practice	-0.12	0.07	*	-0.22	0.09	**
Free-Standing practice	0.26	0.21		-0.02	0.25	
University practice	-0.18	0.08	**	-0.14	0.1	
Small Practice (1-3)	0.17	0.06	**	0.11	0.07	*
Multi-Specialty Practice	-0.02	0.05		0.12	0.05	**
Part Owner	-0.25	0.05	***	-0.13	0.05	**
% Managed Care	0.00	0.00		0.00	0	
Employed Spouse/Partner	-0.02	0.06		0.04	0.05	
US Med School	-0.28	0.07	***	-0.26	0.09	***
Age	0.02	0.07		0.06	0.06	
Age Sq.	0.00	0.00		0.00	0	
Percent Urban	0.00	0.00	*	0.00	0	**
Hispanic	-0.03	0.09		0.04	0.1	
Asian	-0.05	0.07		0.01	0.08	
Black	-0.10	0.10		0.25	0.1	**
Log Physicians per 100,000 Residents	-0.03	0.10		-0.22	0.1	**
Log Median Household Income	-0.07	0.07		0.11	0.06	*
N	445			393		
Adj R^2	0.14			0.13		

Note: Marginal effects reported. All models include workers with 25 or fewer years of potential experience. The dependent variable is 1 if the physician moved practices within the last 10 years, and 0 otherwise. White-Huber heteroskedasticity-adjusted standard errors reported. * significant at 10%, ** significant at 5%, *** significant at 1%.

In Table 2.10 we use aggregate data on the number of physicians per capita from all 50 states to estimate whether the restrictiveness of NCAs has an effect on the geographic distribution of physicians. We find some evidence that this is the case for the 10 states with the most restrictive NCA laws, but that there appears to be no statistically significant effect on aggregate supply outside of those states. Specifically, we find that the number of general physicians per capita is about seven percent lower for states in the highest quintile of Bishara scores. We control for income and sources of insurance that affect demand for physician services, including the percentage of residents living below the poverty line, the percent above 65 years old who are eligible for Medicare, and the percent of residents with employer-provided health insurance. We find that the mean effect is about the same for urban and rural areas, although the standard errors increase when we change the dependent variable to be physicians per capita in urban and rural areas in Models 16 and 17, respectively, and the coefficients in these models are not statistically significant at the 5% level. An F-test that the per capita supply of physicians is the same in states among the highest and lowest quintiles of the Bishara score is strongly rejected in Model 15, 16, and 17, with p-values between 0.03 and 0.04.

Given the strong evidence of assortative matching within states and selection into states based on NCAs, we cannot estimate the causal effect of NCAs on mobility because of the endogeneity caused by latent preferences for occupational mobility that give rise to the observed selection. However, we can estimate the combined effects of selection into NCAs and any potential causal effect of NCAs on mobility. This estimate is unbiased if the acceptance of an NCA in a given state is an effective proxy for latent preferences for mobility, so that the error term is uncorrelated with included variables. The individual effects themselves are not identified, but the combined effect may be useful for policymakers. Firms may benefit from using NCAs either as a mechanism to select physicians based on unobserved preferences or to causally affect mobility, both of which provide value to the firm and may contribute to the justification of the legality of their use. Table 2.11 shows our estimates

Table 2.10: State Selection Models

Dependent Variable	(14) GPs PC, Total Coeff.	S.E.		(15) GPs PC, Total Coeff.	S.E.		(16) GPs PC, Urban Coeff.	S.E.		(17) GPs PC, Rural Coeff.	S.E.	
Log Per Capita Income	0.56	0.13	***	0.55	0.11	***	0.33	0.15	**	0.29	0.20	
% of Population in Poverty	-0.01	0.01		-0.02	0.01		-0.03	0.02	*	-0.02	0.02	
% of Population Over 65	0.04	0.01	***	0.04	0.01	***	0.05	0.01	***	0.00	0.01	
% of Population with Emp. Prov. Insur.	0.00	0.01		0.00	0.01		0.00	0.01		-0.01	0.01	
Bishara Score	-0.07	0.06										
State in 1st Quintile of Bishara Scores				-0.07	0.03	**	-0.07	0.05		-0.08	0.05	*
State in 5th Quintile of Bishara Scores				0.03	0.05		0.06	0.05		0.05	0.06	
Const.	-1.51	1.45		-1.50	1.30		0.88	1.63		1.74	2.36	
N	50			50			50			49		
Adj R^2	0.52			0.55			0.42			0.33		
P-value of F-test 1st Quintile = 5th Quintile				0.04			0.03			0.04		

Note: Dependent variable in Models 14-15 equals general physicians per capita in state. Dependent variables in Models 16-17 equal general physicians per capita in urban and rural areas of the state, respectively. Independent variables "% of Population in Poverty" and "% of Population Over 65" are calculated to match the geographic measure of the dependent variable. States in the 1st quintile of Bishara scores are: FL, KS, CT, IL, ID, UT, IA, MI, NJ, and KY. States in the 5th quintile of Bishara scores are: NY, GA, NE, WV, MT, OK, AK, AR, CA, and ND. NJ not included in rural model. White-Huber heteroskedasticity-adjusted standard errors reported. * significant at 10%, ** significant at 5%, *** significant at 1%. Data sources: Bishara (2011), Johnson and Larson (), Decennial Census Data, Kaiser Family Foundation.

of the combined effects of selection and any potential causal effect of NCAs on mobility. Model 20 shows that among physicians with fewer than 25 years of potential experience, those who chose to accept NCAs were 19 percentage points less likely to have switched practices within the past decade than physicians without NCAs. Our theoretical model suggests that there may be a causal relationship, but the magnitude of any such relationship, if it exists, is unidentified.

Whereas non-competes may be associated with differences in mobility that affect the extensive margin of labor supply in local markets, there may also be an intensive margin relationship. Table 2.12 presents estimates of the static elasticity of labor supply for physicians with and without non-compete agreements. Note that we do not have data on spouses' earnings, so we can only control for spousal employment, and we do not know about unearned income. In Model 23 we estimate the elasticity of labor supply to be about -0.164 for physicians without non-competes, suggesting that physicians make marginal decisions on the backward bending portion of the labor supply curve. We find evidence that non-compete agreements are associated with an increase in the absolute value of the elasticity of labor supply, to about -0.265. Model 24 uses deciles of the physicians hourly earnings to instrument for wages, which has been shown to reduce attenuation bias from measurement error in wages.[15] In the IV model we still find a statistically significant difference. When we estimate similar models using the Bishara score rather than a dummy variable for NCAs, we find no significant relationship between the strength of state laws and the elasticity of labor supply. It is also notable in Models 23 and 24 that we find a significant difference in the number of hours worked depending on whether a physician has an NCA. Those with NCAs worked over 800 hours more annually

[15]See, for example, Blau and Kahn (2007)

Table 2.11: Logit Mobility Models

Dependent Variable	(18) Full Sample Moved 10 Yrs Coeff.	S.E.		(19) $PE < 10$ Moved 10 Yrs Coeff.	S.E.		(20) $PE < 25$ Moved 10 Yrs Coeff.	S.E.		(21) $PE < 25$ Moved 5 Yrs Coeff.	S.E.		(22) $PE \geq 25$ Moved 10 Yrs Coeff.	S.E.	
State Restr. Score×Non-Compete	-0.08	0.05		-0.26	0.11	**	-0.19	0.06	***	-0.13	0.04	***	0.24	0.09	**
Specialist: Internal Medicine	0.06	0.04		0.19	0.09	**	0.10	0.05	*	0.10	0.04	**	-0.10	0.07	
Specialist: Pediatrics	0.03	0.04		0.15	0.08	*	0.06	0.04		0.04	0.03		-0.06	0.07	
Specialist: Secondary Specialty	0.05	0.04		0.30	0.09	***	0.07	0.05		0.06	0.04		0.01	0.06	
Office-based practice	-0.13	0.04	***	-0.04	0.09		-0.17	0.05	***	-0.10	0.04	**	-0.05	0.09	
Free-Standing practice	0.26	0.14	*	0.39	0.21	*	0.23	0.16		0.10	0.13		0.41	0.35	
University practice	-0.16	0.05	***	0.02	0.12		-0.18	0.05	***	-0.09	0.03	***	-0.06	0.11	
Small Practice (1-3)	0.13	0.04	***	0.01	0.08		0.14	0.05	***	0.06	0.04	*	0.15	0.07	*
Multi-Specialty Practice	0.05	0.03		0.03	0.07		0.06	0.04		0.04	0.03		0.02	0.06	
Part Owner	-0.20	0.03	***	-0.15	0.07	**	-0.20	0.04	***	-0.13	0.03	***	-0.20	0.06	***
% Managed Care	0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00	*
Employed Spouse/Partner	-0.01	0.03		-0.05	0.07		0.01	0.04		-0.01	0.03		-0.05	0.06	
US Med School	-0.24	0.05	***	-0.27	0.12	**	-0.28	0.05	***	-0.05	0.04		-0.19	0.10	*
Percent Urban	0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00	
Log Physicians/100,000 Residents	-0.08	0.07		-0.07	0.13		-0.14	0.08	*	-0.13	0.06	**	0.10	0.13	
Log Median Household Income	0.00	0.04		0.04	0.09		0.01	0.05		0.01	0.04		-0.05	0.07	
State: PA	-0.04	0.06		-0.04	0.12		-0.03	0.07		0.11	0.07	*	-0.02	0.12	
State: CA	-0.02	0.05		-0.08	0.11		-0.06	0.06		-0.02	0.04		0.11	0.12	
State: TX	-0.01	0.06		-0.15	0.10		-0.03	0.06		0.00	0.05		0.11	0.15	
State: IL	0.03	0.06		-0.04	0.12		0.09	0.08		0.10	0.07		-0.03	0.12	
N	1144			318			878			878			266		
Adj R^2	0.124			0.134			0.134			0.128			0.174		

Note: Marginal effects reported. Model 18 includes the full sample, Model 19 includes only physicians with potential experience less than 10 years, Models 20 and 21 includes physicians with potential experience less than 25 years, and Model 22 includes physicians with potential experience greater than or equal to 25 years. The dependent variable in Models 18-20 and Model 22 is 1 if the physician moved practices within the last 10 years, and 0 otherwise. The dependent variable in Model 21 is 1 if the physician moved practices within the last 5 years, and 0 otherwise. White-Huber heteroskedasticity-adjusted standard errors reported. * significant at 10%, ** significant at 5%, *** significant at 1%. All models also include age and race controls.

than physicians without NCAs, which is consistent with the claim that physicians with NCAs invest more effort in building relationships with their patients.

2.6 Conclusion

There is large legal literature on non-compete agreements and they have been the subject of considerable controversy, but there has been little systematic analysis on their use or impact on labor markets. While our sample is limited to family practice physicians and pediatricians in five states, this paper makes three important contributions to the literature. First, it provides new concrete evidence of widespread use of non-competes in physician groups in general practice and pediatrics that is broadly consistent with claims about their growing use. Even in California, where NCAs have little legal restriction, over 30% of physicians in our sample reported being subject to an NCA.

Second, our analysis of the adoption of non-competes suggests they are being used in ways consistent with economic theory, and that public policy has a role in affecting outcomes. Use of non-competes is higher in office-based practices and for employees. At the same time, the use of non-competes is consistent with differences in state level policies. We find substantial evidence that physicians assortatively match with practices based on the use of NCAs, and that states with extremely restrictive laws have a lower aggregate supply of general physicians per capita.

Third, we find evidence that non-competes substantially affect labor market outcomes. Wages of physicians with non-compete agreements are about 11 percent

Table 2.12: Elasticity of Labor Supply Models

Dependent Variable: Annual Hours	(23) GLS	(24) IV	(25) GLS	(26) IV
Log Wage	-358.52 ***	-415.44 ***	-423.42 ***	-472.14 ***
	(82.53)	(61.42)	(77.82)	(68.72)
Log Wage×Non-Compete	-240.10 **	-193.93 **		
	(114.22)	(90.94)		
Non-Compete	1026.69 **	832.48 **		
	(487.56)	(288.14)		
Log Wage×Bishara Score×Non-Compete			-169.80	-157.87
			(160.59)	(143.30)
Bishara Score×Non-Compete			743.28	695.07
			(680.54)	(609.79)
Employed Spouse	-211.78 ***	-211.87 ***	-218.09 ***	-218.44 ***
	(41.87)	(42.74)	(42.68)	(41.73)
Children Under 6	-181.30 ***	-179.84 ***	-170.85 ***	-167.82 ***
	(57.39)	(54.56)	(57.81)	(57.54)
N	750	750	729	729
R^2	0.282	0.281	0.278	0.277
Elasticities (At mean)				
Own Wage, Without Non-Compete	-0.164	-0.190	-0.193	-0.215
Own Wage, With Non-Compete	-0.265	-0.270	-0.263	-0.280

Note: White-Huber heteroskedasticity-adjusted standard errors reported in parentheses. All models include controls for age, age squared, physician specialty, office-based practice, small practice, ownership status, US medical school graduate dummy, three race dummies, percent of patients from urban zip codes, log physician to population ratio in zip code, and state dummies. Sample includes physicians who reported between 200 and 4000 annual hours worked and are less than 65 years old. * significant at 10%, ** significant at 5%, *** significant at 1%.

higher than those without on average, and about 17 percent higher in states with the most strict legal enforceability of non-competes. To our knowledge, these estimates provide the first empirical evidence on the wage effects of non-compete agreements in any labor market. We also find that these higher wages are passed on to consumers in the form of significantly higher prices for services in states with more restrictive NCA laws. We estimate the relationship between the use of NCAs and occupational mobility, although the causal effect is unidentified in our data. We find that physicians with non-compete agreements in strict states were 19 percentage points less likely to have switched practices within the previous ten years than those without non-competes. This estimate is the combined effect of assortative matching and any potential causal effect of NCAs on mobility. It suggests that, regardless of whether there is a causal relationship, firms may benefit from the use of NCAs by either facilitating the selection of physicians who plan to remain at the practice or causally affecting labor mobility. Evidence on effects of NCAs on the dynamic wage paths of physicians suggests that NCAs increase the monopsonistic bargaining power of firms, which affects changes in wages over time, but this effect is offset in part by a steeper initial wage path, consistent with an increase in investment in human capital accumulation by firms.

Taken together, these findings suggest that state policies regarding non-competes play an important role in shaping the organization and operation of physician labor markets. In particular, state-level legislation on non-compete agreements can affect the overall supply of physicians per capita in a state as well as the number of hours that physicians work, the intensive margin of labor supply.

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APPENDIX A

CHAPTER 1: IMPUTATION OF OUTSIDE OPTION WAGE

The structural models that do no rely upon reported wages from non-fishing jobs as the outside option use instead imputed wages from 2003-2009 CPS data. The wage equation used to predict wages for the sample of workers is presented below. In all of the results presented in the paper, the outside option is assumed to be full-time, non-temporary employment. In addition to the variables shown below, the model also includes state effects, year effects, quarter of year effects, and interation terms between temporary worker and quarter of the year.

Summary statistics of the imputed wages are presented in Table A.2. The average and median imputed wage was $12.26 per hour.

Table A.1: CPS Wage Imputation Model

Dependent Variable: Log(Hourly Wage)		
	Coeff.	S.E.
Potential Experience	0.0339 ***	[0.0004]
Potential Experience Sq.	-0.0006 ***	[0.0000]
MSA	0.0404 ***	[0.0026]
Less than HS	-0.3902 ***	[0.0047]
HS Graduate	-0.2063 ***	[0.0041]
Some College	-0.1396 ***	[0.0041]
Black, NH	-0.1582 ***	[0.0039]
Asian	-0.0999 ***	[0.0079]
Hispanic	-0.1368 ***	[0.0033]
Other Race	-0.0268 ***	[0.0039]
Part-Time Worker	-0.3560 ***	[0.0041]
Near Full-Time Worker	-0.2007 ***	[0.0055]
Temporary Worker	-0.1841 ***	[0.0224]
Blue Collar Job	0.07916 ***	[0.0024]
White Collar Job	0.2515 ***	[0.0035]
N Obs.	205,231	
R^2	0.284	

Table A.2: Summary Statistics of Imputed Wages

Mean Hourly Wage	12.26
Minimum Hourly Wage	6.13
10th Percentile Hourly Wage	9.94
Median Hourly Wage	12.26
90th Percentile Hourly Wage	14.57
Maximum Hourly Wage	17.81